

JUNE 1961

Current Anthropology

A WORLD JOURNAL OF THE SCIENCES OF MAN

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CURRENT ANTHROPOLOGY es una revista en lengua inglesa que aparece seis veces al año. Se dirige a los especialistas del mundo entero dedicados a las disciplinas antropológicas, incluyendo la Antropología social, cultural y biológica, la Lingüística, la Arqueología y la Prehistoria. Los asociados a CURRENT ANTHROPOLOGY forman un grupo cooperativo de especialistas que se comunican conocimientos e ideas a través de esta revista y que aspiran a un pleno y libre intercambio científico en el ámbito mundial. Las instituciones que aceptan asociarse a CURRENT ANTHROPOLOGY asumen la misma obligación. Las dos categorías de asociados reciben la revista y pagan únicamente una cuota nominal (el 20 % del importe de la suscripción), ya que su participación activa supone gastos suplementarios. Los estudiantes de disciplinas antropológicas fundamentales y los investigadores de campos estrechamente ligados con aquéllas pueden ser recomendados por los asociados, y obtendrán la inscripción a precios reducidos (40 % de la tarifa normal). Cualquiera que lo desee puede abonarse a la tarifa corriente.

Historia del proyecto

Desde su fundación en 1941, la Wenner-Gren Foundation for Anthropological Research se dio cuenta de la importancia de los intercambios entre los investigadores de los distintos países en el campo de las ciencias del hombre. Por ello, en 1952, la Wenner-Gren Foundation acordó patrocinar un Symposium Internacional de Antropología, del que resultó la publicación de *Anthropology Today*, *Appraisal of Anthropology Today* y el *International Directory of Anthropological Institutions*. A continuación de estas obras, y a título experimental, en 1955 fue publicado un *Yearbook of Anthropology*, una parte del cual fue reeditado en 1956 bajo el título de *Current Anthropology*, nombre que hemos tomado para la presente revista. Esta fue creada en 1957 para continuar aquellos propósitos.

La presentación y el contenido de la revista CURRENT ANTHROPOLOGY se discutieron en sucesivas conferencias y por correspondencia con los especialistas del mundo entero. En el curso de la conferencia inaugural de Burg Wartenstein (Austria),

como centro europeo de la Wenner-Gren Foundation, se formularon los siguientes principios generales:

1. La revista tratará de todos los problemas interesantes referentes a las diversas disciplinas antropológicas, y reunirá los hechos e ideas importantes, facilitando el intercambio internacional.

2. Tenderá a la síntesis integradora.

3. Constituirá para los especialistas un instrumento de trabajo apto para intercambiar información sobre sus actividades en curso.

El año siguiente, gracias al concurso de nuestros colegas, después de reuniones celebradas en todas partes del mundo, los principios enunciados tomaron la forma del plan que presentamos a continuación.

El estatuto de los asociados

CURRENT ANTHROPOLOGY agrupa a especialistas e instituciones sin tener en cuenta la nacionalidad ni las tendencias políticas. A cambio de la cuota especial que les ha sido concedida, se espera de los asociados que colaboren buscando nuevos miembros, mandando artículos y comunicando noticias y otros materiales que puedan interesar a los restantes asociados. El editor cuenta igualmente con ellos para obtener comentarios acerca de los artículos de fondo.

Contenido

CURRENT ANTHROPOLOGY publica dos clases de materiales: por una parte, los artículos de fondo; por otra, noticias e informaciones diversas. La revista publicará forzosamente artículos que podrían haber encontrado lugar en otra parte. No obstante, su amplio campo geográfico y teórico hará que sea poco probable el que se produzca duplicidad de temas con otras revistas. La función de CURRENT ANTHROPOLOGY, como instrumento de intercambio entre todos los especialistas, responde a una fórmula original.

Artículos de fondo

Estos artículos tratarán de problemas generales, y han de dar una visión de conjunto de cuanto se sepa sobre un tema importante de las ciencias del hombre. Es preferible nuevo material y nuevas soluciones referentes a los puntos de interés creciente en Antropología a las síntesis de lo ya conocido; pero el nuevo material ha de estructurarse en el marco de lo sabido. Un artículo de este carácter ha de contener el estudio de textos, datos, resultados de investigaciones, métodos, etc. y sus límites pueden ser cronológicos, geográficos o basarse en otro criterio cualquiera. Por lo general debe incluir el esquema básico o un resumen sumario del desarrollo histórico del tema, que puede servir como punto de partida. El tema debe ser ambicioso, tratado ampliamente, ilustrado con casos concretos, interpretativo y atento al desarrollo futuro. Debe contener extensa bibliografía. Por ser el autor un especialista que se dirige a especialistas en otras ramas, ha de tratar de ser claro en su presentación, especialmente en la terminología. El artículo puede ser tan largo como lo exija la ma-

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Six Selected as Candidates for 1961 Viking Medal

15 Other Anthropologists Repeatedly Named

ASSOCIATES HONOR TOTAL OF 70 COLLEAGUES

The Six Candidates

Evans-Pritchard, Edward E. Institute of Social Anthropology, 11 Keble Rd., Oxford, England. Professor of Social Anthropology, Oxford. Social anthropology. Africa, Arab lands.

Heine-Geldern, Robert. Inst. für Völkerkunde, Reitschulgasse 2, Vienna 1, Austria. Prof., U. Vienna, retired. Ethnol., arch. India, S. E. Asia, Oceania, China (Mesol. to Han period), megalithic cul., interrelations Old and New World.

Leakey, Louis S. B. Director, Coryndon Memorial Museum, Box 658, Nairobi, Kenya. Paleontology, Bantu linguistics. East Africa, Europe.

Lévi-Strauss, Claude. 2, rue des Marronniers, Paris 16, France. Prof., Ecole Pratique des Hautes Etudes, Paris. Social anthropology, kinship, social org., religion, mythology, art. comp. anth. N. & S. America.

Métraux, Alfred. Official of UNESCO, Dept. Soc. Sci., Place de Fontenoy, Paris 7, France. Cultural anthropology, religion, culture areas, history of native tribes, acculturation. South America.

Tax, Sol. Professor of Anthropology, U. Chicago, Chicago 37, Ill., U.S.A. Social & cultural anthropology, applied anthropology. Soc. & Cul. change, soc. org., econ. anth., gen. theory. Mesoamerica, N. Amer. Indians.

By March 11, the last day on which nominations for the 1961 Viking Fund Medal could be counted, the six anthropologists listed above had been named by 52% of the 228 Associates in *CURRENT ANTHROPOLOGY* who submitted nominations. They are the Candidates from among whom the Medalist will be chosen.

Biographical information about the six Candidates, and statements about the contribution that each of them has made to world anthropology, are given on pages 198-99.

As reported in *CA* (May 1960, p. 257; April 1961, p. 136), the three medals which during the past fifteen years have been awarded annually to scientists chosen by three American societies will

now be replaced by a single medal presented annually on a world-wide basis. On the invitation of Dr. Paul Fejos, President of the Wenner-Gren Foundation, the selection is being made by the Associates in *CURRENT ANTHROPOLOGY*. The 45 medalists previously chosen (see Table) are not eligible for the new single award.

The process of choosing the medalist from among the six Candidates will begin with the receipt of this issue of *CA*. Ballots will be sent to and tabulated at the headquarters of the Wenner-Gren Foundation in New York. The name of the Medalist will be announced in the February 1962 issue of *CA*.

Fifteen Near-Candidates

Other anthropologists who were repeatedly named, and where available, quotations from the statements offered to support their nominations, are:

KAJ BIRKET-SMITH

"... for his life-work in field research, and his contributions to ethnological and cultural theory."

ROBERT J. BRAIDWOOD

"... because of his major contributions to the study of the beginnings of the Neolithic in the Near East; contributions in which he has brought the methods of anthropology to bear upon the archaeological problems of the origin of village life."

(Continued on page 200)

VIKING FUND MEDALS AND AWARDS

Recipients named by 3 North American Societies

- 1946 Alfred V. Kidder, Alfred L. Kroeber, Franz Weidenreich
- 1947 John O. Brew, Earnest A. Hooton, Robert H. Lowie
- 1948 Alex D. Krieger, Adolph H. Schultz, John R. Swanton
- 1949 Wm. King Gregory, Hallam L. Movius, Jr., George P. Murdock
- 1950 Emil W. Haury, Clyde Kluckhohn, Wilton M. Krogman
- 1951 Carleton S. Coon, Ralph Linton, Frank H. H. Roberts, Jr.
- 1952 Alfonso Caso, Julian H. Steward, William L. Straus, Jr.
- 1953 Melville J. Herskovits, T. Dale Stewart, Gordon R. Willey
- 1954 William W. Howells, Robert Redfield, Wm. Duncan Strong
- 1955 A. I. Hallowell, W. E. Le Gros Clark, J. Eric S. Thompson
- 1956 Junius B. Bird, Fred Eggan, Mildred Trotter
- 1957 Raymond A. Dart, James B. Griffin, Margaret Mead
- 1958 Raymond W. Firth, Jesse D. Jennings, Henri V. Vallois
- 1959 William W. Greulich, Irving Rouse, Leslie A. White
- 1960 Samuel K. Lothrop, Leslie Spier, Sherwood L. Washburn

Candidates named by Associates in *CURRENT ANTHROPOLOGY*

- 1961 Edward E. Evans-Pritchard, Robert Heine-Geldern, Louis S. B. Leakey, Claude Lévi-Strauss, Alfred Métraux, Sol Tax
-

E. E. EVANS-PRITCHARD, Professor of Social Anthropology at the University of Oxford and Fellow of All Souls College, Oxford, was born in England in 1902. Educated at the University of Oxford (M.A.) and the University of London (Ph.D.), he has held positions at the London School of Economics, Egyptian University in Cairo, the University of Cambridge, and the University of Chicago. He has done field work in the Sudan, Congo, Ethiopia, Kenya, and Upper Egypt. Among his major publications are *Witchcraft, Oracles and Magic among the Azande* (1937); *The Nuer* (1940); *The Political System of the Anuak* (1940); *African Political Systems* (editor, with M. Fortes, 1940); *The Sanusi of Cyrenaica* (1949); *Kinship and Marriage among the Nuer* (1951); *Social Anthropology* (1951) and *Nuer Religion* (1956).



His chief interests are Negro Africa (especially East Africa) and Arab areas; comparative political institutions, comparative ritual, and the history of anthropological theory.

Associates write:

"For his contribution to theories of ecology and social systems through studies of the Nuer; his analytical and eminently reasonable discussions of un-reasoning activities in magic and religion . . . an inspirer of great work in others.

"His work has essentially set the standard for excellence in ethnography: careful empirical investigation, guided by important general ideas. I would say he is the greatest living ethnographer."

"With the publication of *Nuer Religion*, he has completed a cycle of studies on one group that has few parallels in Africa or the rest of the world. The quality of his other ethnographic and historiographic work is well known."

ROBERT HEINE-GELDERN is Professor Emeritus, University of Vienna; Member of the Austrian Academy of Sciences; Research Associate, American Museum of Natural History; and Secretary General of the International Committee on Urgent Anthropological and Ethnological Research. Born in Austria in 1885, he was educated at the Universities of Vienna and Munich, graduating as a Ph.D. from Vienna.

Besides the University of Vienna and the American Museum of Natural History, he has held positions at New York University, Columbia University, School of Asiatic Studies in New York, University of California, and the University of Chicago. Some of his publications are *Das Tocharerproblem und die Pontische Wanderung* (1951), *Some Problems of Migration in the Pacific* (1952), and *Die asiatische Herkunft der südamerikanischen Metalltechnik* (1954).

The main subjects of his research and publication have been: ethnology of Southeast Asia and Polynesia; the prehistoric archaeology of Iran, India, China, and Southeast Asia; art of Southeast Asia; origin and early spread of writing; meaning and distribution of megalithic monuments; problem of trans-Pacific cultural relations; and urgent research on vanishing cultures and languages.

Associates Write:

"He has been the leading scholar in Southeast Asian ethnology and archaeology for more than forty years, and his publications are known to all anthropologists in these fields.

"In the field of pre-Columbian relations between the Old World and the New, Heine-Geldern has become foremost. Equally famous are his studies on the ethnology of Oceania.

"Heine-Geldern is a perfectionist. In an era in which students are inclined to offer hypotheses without adequate proof, the awarding of the Viking Fund Medal to a man who has supported his brilliant ideas with painstaking accuracy and detailed research should have a salutary effect."

" . . . the first scholar who established a valid theory covering all aspects of megalithic cultures and their affinities. He has shown cultural connections and migrations between Europe and East Asia, as well as the strong influence of Far Eastern cultures on ancient American high cultures."

"His activities in the International Committee on Urgent Anthropological and Ethnological Research further a very important part of anthropological research."

LOUIS S. B. LEAKEY is Director of the Coryndon Memorial Museum, Nairobi, Kenya, and President of the Pan-African Congress on Prehistory. Born in Kenya in 1903, he was educated at the University of Cambridge (M.A., Ph.D.). He has done extensive field work in East Africa on native cultures as well as on the origin of man. He is a Fellow of the Royal Anthropological Institute, of the British Academy, and of the Geological Society of London. His major publications include *The Stone Age Races of Kenya* (1935); *The Miocene Hominoidea of East Africa* (1951, with Le Gros Clark); and *Olduvai Gorge: A Report on the Evolution of the Handaxe Culture in Beds I-IV* (1951).

His major anthropological interests are general physical anthropology, human paleontology, archaeology, linguistics, and folklore; his region of specialization embraces East and South Africa.



Associates write:

"He has, over a considerable period of years, contributed to all fields of anthropology, and has combined high

competence in the fields of prehistoric archaeology, human paleontology, and ethnology. While his major contributions have been in the field of human paleontology, his broad range of scholarly accomplishment has added a richness to his observations and depth to his interpretation of prehistory."

" . . . because of his great contribution in recovering the *Zinjanthropus* skull; his excellent writings about early African beginnings; his knowledge of the native peoples of East Africa; his aid to the government in solving problems of East African natives."

"An anthropologist who practices 'participation' in living African cultures as well as in the prehistoric cultures, reaffirming the principle of human brotherhood."

CLAUDE LÉVI-STRAUSS, Professor of Anthropology at the École Pratique des Hautes Etudes, Paris, was born in 1908 in Belgium. Educated at the University of Paris (Lic. Droit, Philosophie), he has held positions at the University of São Paulo and the New School for Social Research in New York, as well as at the University of Paris. In 1946 and 1947 he was French Cultural Attaché in the United States. He has carried out field work in the Mato Grosso region of Brazil and in the southern Amazon. Among his important publications are *Les structures élémentaire de la parenté* (1949); *Race and History* (1952); *Results of the Conference of Anthropologists and Linguists* (1953, with others); and *Anthropologie structurale* (1958).

His chief interests are social anthropology, kinship, and

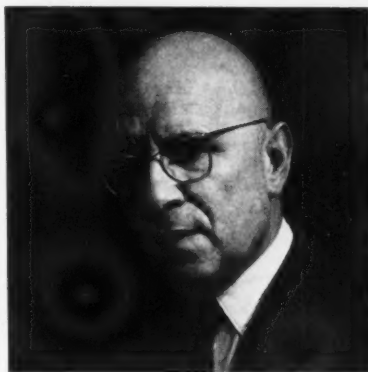
social organization; religion and mythology; art; and the comparative anthropology of North and South America.

Associates write:

"Probably the foremost anthropological theorist of the present day, who has stimulated thinking in many fields within anthropology, strengthened the links with related disciplines, and added to the stature of cultural anthropology as a whole."

"One of our most brilliant anthropologists, an original thinker whose contributions to the understanding of the structural aspects of such different phases of culture as social organization and folklore have been developed out of his equally significant ethnographic works."

ALFRED MÉTRAUX, Programme Specialist in the Department of Social Sciences, UNESCO, Paris (in charge of "Human Rights"), is also Professor of South American Anthropology at the École Pratique des Hautes Etudes, 6th section, Paris. Born in 1902 in Switzerland, he holds the following degrees: Licencié ès-lettres (Sorbonne); Diplômé de l'École des Langues Orientales; Diplômé de l'École Pratique des Hautes Etudes (Paris); and Docteur ès-lettres (Sorbonne). Former Director and founder of the Instituto de etnologia de la Universidad Nacional de Tucuman (Argentina), he has also held positions at the Bishop Museum in Honolulu, Yale University, the University of California, and the Bureau of American Ethnology. He has done field work in the Gran Chaco; among the Uro Indians of the Bolivian highlands; on Easter Island; in Haiti; and in central Brazil. Some of his major publications are *The Ethnology of Easter Island* (1940); *Myths and Tales of the Pilaga*



Indians (1942); *Making a Living in Marbial* (1945); *L'Ile de Pâques* (1950); *Easter Island*, 1957; and *Le vaudou Haïtien* (1959; *Voodoo in Haiti*, 1959).

His chief regional interests are culture diffusion in South America and the history of South American Indians; his theoretical interests center on religion and political structure.

Associates write:

"It is primarily on the grounds of his very diversified interests, and most particularly of his contributions to the thinking of anthropologists and social scientists (students as well as scholars) that he seems most deserving of recognition in the year in which the Viking Fund Medalist is first to be chosen internationally. He is also one of the very few professional anthropologists active today who bridges anthropological theory and practice in Europe and the Americas. His current work in UNESCO depends on his ability to function internationally as an anthropologist.

"As a person who, with great verve and skill and understanding for the viewpoints of scholars of different traditions, has fostered communication among our professional colleagues and with government agencies and private groups of interested persons, his contribution to the anthropology of our generation is a very considerable one."

SOL TAX is Professor of Anthropology at the University of Chicago, and Editor of *Current Anthropology*. Born in Chicago in 1907, he was educated at the University of Wisconsin (Ph.B.) and the University of Chicago (Ph.D.). He has done field work among the Fox Indians (U.S.A.), in North Africa, among the Indians of Guatemala and Chiapas. Among his major publications are "The Social Organization of the Fox Indians" (1937), "Action Anthropology" (1952), and *Penny Capitalism: A Guatemalan Indian Economy* (1953). He served as editor for the 29th International Congress of Americanists (1949), and (with others) for the Wenner-Gren Foundation's 1952 International Symposium on Anthropology, as well as for the series *Evolution after Darwin* (1960). At present he is co-ordinating a program to help American Indians express their views and needs.

His major anthropological interests include social and cultural anthropology and applied anthropology; his regions of specialization are Mesoamerica and North America.

Associates write:

"He has done more than any single person to bring together the family of anthropologists as scientists who purport no prejudices, regardless of nation, culture, religion, or race. Not only has he devised a method of exchanging in-

formation through the organ *CURRENT ANTHROPOLOGY*, but he has brought together into meetings anthropologists who normally would have no contact with each other. He has served as a good-will agent, a diplomat, and a true scientist in this task. In addition to this important role in disseminating information through the organization of *CURRENT ANTHROPOLOGY*, he has an outstanding record in scientific publications based on extensive field work in Latin America.

"His efforts to organize symposia bringing together authorities from the world over are most recently epitomized in the Darwin Celebration and the resulting three-volume publication. His constant concern for the profession of anthropology is always foremost in his actions, and can be shown by his accomplishments as editor of the *American Anthropologist* and president of the American Anthropological Association."

"... for many years an outstanding contributor to our science in research, in teaching, and in editing. The successful organization of CA and a world-wide association of anthropologists is largely due to his initiative and energy."

"... for his distinguished contributions to Middle American anthropology, for his editorial inventiveness, and for his present activities in behalf of North American Indians."

(Continued from page 197)

HENRI BREUIL

"... parce que tante son activité, depuis 1897, a été entièrement consacrée à l'étude de la Préhistoire et de l'Anthropologie préhistorique, que tous les travaux qu'il a effectués et publiés pendant ces soixante trois années sont à la base de tant les recherches préhistoriques actuelles. On peut considérer qu'il est le dernier survivant de l'équipe de Préhistoriens, que, au début de notre siècle, a réellement commencé le classement de connaissances jusque là très fragmentaires, en créant ainsi cette science nouvelle."

J. G. D. CLARK

"... the founder of the school of Economic Prehistory. His studies have opened completely new regions in prehistoric research; they have given us a completely new picture of primitive societies. He has formed many pupils who continue his work. In my opinion, he is a worthy successor to V. Gordon Childe."

VOJTĚCH FETTER

C. DARYLL FORDE

"... for his Ethnographic Survey of Africa series."

MEYER FORTES

"... because of his fundamental contributions to the development of unilineal descent theory."

STANLEY M. GARN

JOSEPH H. GREENBERG

"... a scholar who has made first-rate contributions to the study of kinship and kinship theory; to the ethnology of South America and to the culture history of Mesoamerica." "... an outstanding example of a truly international anthropologist who has dedicated his life to the discipline, and who has given his time and

knowledge unsparingly to his colleagues and students, with no expectations of self-aggrandizement."

EDMUND R. LEACH

ANDRÉ MARTINET

"He has been for many years one of the most active supporters of Structural Linguistics, but he is aware of the dangers of an excessive and blind formalism; he is familiar with the methods of diachronic as well as of synchronic studies. He has spent many years in the United States and has a large audience both in America and Europe."

"Le livre *Economie des changements phonétiques* pose les fondements de la linguistique diachronique explicative."

OSWALD F. A. MENGHIN

"I know of nobody who has tried so much to foster a new synthesis of the diverging researches and materials of general anthropology. Though his work, both theoretical and in the field, was until World War II mainly confined to the Old World, his very stimulating studies and publications have also during the last decennium included America (field work and the fostering of new centers of research in South America) to an extent that deserves respect and admiration."

KENNETH L. PIKE

"His book *Language*, notwithstanding the fact that, as with every scientific book, one could disagree with many points and possibly with the whole underlying idea that behaviour can be considered an articulated process and that it can be analyzed into discrete units, is to my knowledge the first general theory which tries to unite speech with other activities of man—and does so, not in terms of a description of the behaviour pattern of this or that tribe, but generally. His really excellent textbooks join the advantages of clear ideas, practical reasoning, and precise discussion of the matter at hand. They are widely known and highly appreciated."

MORRIS SWADESH

"... for his work in lexicostatistics. His basic papers on the subject have stimulated great interest and have resulted in an extensive literature. While the subject is primarily linguistic, it has major implications for cultural anthropology in general. While it focusses on problems of culture history, it also has major implications for structural and functional studies. It therefore contributes to the synthesis of synchronic and diachronic approaches to the study of culture, which many believe to be one of the most important general theoretical developments in modern cultural anthropology."

Forty-Nine Others Honored

Other anthropologists honored by their colleagues were: MAJOR R. C. ABRAHAM, GEORGE A. AGOGINO, KIZAMON ARIGA, H. G. BARNETT, THOMAS BARTHEL, IGNACIO BERNAL, FRANÇOIS BORDES, J. DESMOND CLARK, TH. DOBZHANSKY, LOREN C. EISELEY, JAMES A. FORD, MAURICE FREEDMAN, JULIO DE LA FUENTE, JOHN P. GILLIN, MAX GLUCKMAN, CARL E. GUTHE, GERHARD HEERER, R. F. HEIZER, W. C. OSMAN HILL, HARRY HOIJER, SIR JULIAN HUXLEY, ROMAN JAKOBSON, GEORGE KOSSACK, WESTON LA BARRE, PIA LAVIOSA-ZAMBOTTI, LI CHI, SIGVALD LINNÉ, SETON LLOYD, M. L. MALLOWAN, J. ALDEN MASON, GEORG MORGENSTIERNE, ARTHUR E. MOURANT, WILHELM EMIL MÜHLMANN, KENNETH P. OAKLEY, MORRIS E. OPLER, LUIS PERICOT-GARCIA, RICHARD PITTONI, GEORGE I. QUIMBY, AUDREY I. RICHARDS, JOHN M. ROBERTS, EUGÈNE SCHREIDER, HARRY L. SHAPIRO, EDWARD H. SPICER, GEORGE L. TRAGER, TUNG TZANG-PING, RAYMOND VAUFREY, W. LLOYD WARNER, MONICA WILSON, and F. E. ZEUNER.

Anthropological Studies of Complex Societies¹

by *S. N. Eisenstadt*

I

THE PURPOSE OF THIS PAPER is to analyse some of the problems arising out of the application of the methods and approaches developed in social anthropology to the study of more complex societies—whether “historical” or modern societies or parts thereof. We hope to show both the contributions and limitations of some of the concepts and tools of social anthropology in the study of more complex societies. We shall not deal here with general anthropological approaches to such studies (Mandelbaum 1956), nor with such general concepts as “culture” or such general problem areas as “culture and personality,” which have developed mainly in anthropology. Nor shall we be concerned with studies of tribal or peasant communities by anthropologists or other scientists who have employed concepts and techniques common to many behavioral sciences.

We shall, rather, concern ourselves mainly with the potential contributions to the study of complex societies that have been made by one specific branch of anthropology, namely, social anthropology as it has been developed in England (for a general survey see Evans-Pritchard 1951; Beattie 1956), and to a smaller extent in U.S. and France (Eggan 1950, 1957; Lévi-Strauss 1959). These social-anthropological studies have developed a distinct theoretical model which deals with a certain order of problems or of social phenomena. The development of this model, and of specific rigorous techniques of field work, account for the great impact of

social-anthropological studies on our general understanding of the working of human societies. Out of the application of this model to the study of complex societies, several problems have emerged which may be of great interest from the point of view of general anthropological and sociological theory.

What, then, is this model, and to what kinds of problems is it applied?²

Its most distinctive characteristic is that it combines the description and analysis of social behavior, group structure and institutional settings into one set of inter-related concepts and analytical tools.

On the purely descriptive level, social anthropologists usually deal with observed patterns of social behavior of individuals in different social situations and in different groups, and with statements by individuals of norms which would be appropriate to such different situations (Beattie 1959 and, in more detail, Emmett 1958). However, this description of social behavior is, at the same time, a study and analysis of the structure of groups and societies.

This type of analysis is achieved through the great emphasis on social behavior as related to various norms which are said to be operative in the social structure. Most of the social-anthropological descriptions of social behavior are studies of the ways in which major norms found in these societies are upheld by individuals, of the interrelations between these different norms, and of the ways in which these norms influence and regulate the relations between different groups in society.

Thus we find in these studies, for instance, a great emphasis on the institutional norms directing and restricting individual behavior in the major fields of social life—marriage and family life; economic, political or ritual organizations. Secondly, and in close connection, we find an analysis of the consequences of upholding such norms for social and intergroup relations within a

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² We shall not deal below with the historical development of this model and with the various trends within it, but shall concentrate mainly on its basic and salient features (Evans-Pritchard 1951; Beattie 1956).

given society—e.g., how the marriage of a man from group A to a woman from group B affects relations between these groups. Different institutions and the social activities of individuals are analysed, not only within their own spheres, but also as links between several groups in the society, and as contributing to the continuity of these groups and of the society in general. Thus, for instance, the rules that govern economic or political activities are explained in terms of the “needs” of these groups and of the society as a whole. (For a very pertinent formulation see Fortes 1953 and Gluckman 1956.) Thus, various norms and the patterns of behavior which uphold them are seen mostly as institutional directives upholding the interrelations among different groups in the society and the continuity of the society as a whole. In this way, most anthropological studies combine in one basic model the analysis of social behavior, institutional norms, groups and societies. They explain patterns of social behavior through analysis of group-structure, institutions and “total” societies.

Neither this model, nor the various studies which have been guided by it, have usually dealt with the problems of the kinds of interrelationships between such different variables as social behavior or group and institutional structure—i.e., which variables are dominant or influential in general or in various typical situations. Rather, it has been assumed that all these variables are always very closely interrelated through their contributions to one another, and to the society as a whole through what are usually called their “functions.” (For a partial exception, see Worsley 1956.)

II

This model necessarily raises the question of the mechanisms through which the interrelationships among these variables are maintained. Few examinations of the functional analysis in anthropology (the very type of analysis which stresses interrelationships among different parts of society or culture) have dealt explicitly with this problem (for a partial exception, see Beattie 1959). However, social anthropological studies contain certain postulates, analyses, and descriptions of such mechanisms; and these analyses constitute one of the basic contributions of social anthropology to the study of human society and social processes.

1. The first such mechanism is the interaction of the *same* persons or groups in different situations, an interaction which makes their mutual commitments in one situation or group greatly influence their behavior in others. In the societies studied by anthropologists, the existence of such close relations between the same people in different groups and situations constitutes a very important mechanism regulating social behavior, on the one hand, and intergroup relations on the other (see Barnes 1959). Gluckman has advanced this analysis by showing how close interrelations among the same people in different situations create conflicts between them; and how those conflicts, which seem disruptive, in reality help enhance the solidarity and functioning of the groups and the society through the crosscutting of interests among the same people participating in these varying situations (Gluckman 1956).

2. The second mechanism is the specific type of re-

lation between what may be called “culture” (or rather, values) and ritual symbols, on the one hand, and social relations, on the other hand. Values and symbols are analysed by social anthropologists on two levels. An analysis of these various beliefs and cultural symbols as systems in their own right has recently developed (Forde 1954; Evans-Pritchard 1956). More common, however, is the analysis of values and symbols in relation to various fields of social activity—family, kinship, or political activities. While anthropologists have usually recognized the autonomy and distinctiveness of the sphere of values, they have also emphasized the direct relevance of primordial or sacred symbols and rituals for most types of social activities in the societies they study, and the interlocking of these activities in a way which assures that they are given “meaning” in terms of these symbols and values.

3. The third mechanism, stressed by most of these studies, is the continuous interrelation in these societies of different types of social activities in most groups and situations. Ritual, jural, contractual and political activities—clearly distinguishable from one another (Evans-Pritchard 1955)—are seen as interwoven in most of the situations and groups of these societies, so that each activity directly depends on, articulates with, and is upheld in terms of, the others. Epstein (1954) has put this point thus: “Most of the major social functions are fulfilled by the same small groups.”

These three mechanisms are found to regulate the direct interrelation of social behavior to group and institutional structure, in most parts of the societies studied by social anthropologists. Analytically, the most important aspect of these mechanisms is that they are embedded in the very structure of the major social situations and major groups in the society, and are not organized in special distinct ways or orders. Perhaps this characteristic explains why the social-anthropological model did not explicate the interrelationship of its different variables, and could use as analytical tools concepts *describing* the structure of various groups.

III

These assumptions guided the work of social anthropologists for at least the first two decades, even though there were frequent differences of emphasis among individual anthropologists and among the many trends which developed within social anthropology.

It is beyond the province of this paper to analyse or even summarize all the contributions of social anthropologists, or to discuss the many specific hypotheses they have developed in the application of this model to the different societies which they study. But it would be worthwhile to indicate briefly some major trends or emphases which have developed in social anthropology—without attempting to provide a full list. All of them have stemmed, in a way, from the basic common assumption of their model, but each has emphasized different aspects of it.

1. The numerous well-known studies—which need not be enumerated here—that emphasize the analysis of “total” tribal societies, and very often are identical with analysis of wide ecological communities (e.g. Evans-Pritchard 1940; Krigs 1943).

2. Those studies which are also concerned with total societies, but tend to concentrate on the analysis of these societies from the point of view of one major institutional sphere—marriage and kinship, political structure, or stratification (e.g. Fortes 1945, 1949b; Kuper 1947).

3. An interesting offshoot of the second trend are the general studies of certain institutional groups which are basic constituents of any human society, and some of their basic characteristics. The studies of the domestic group, edited by Goody (1958) and brilliantly summarized by Fortes (1958), are the best recent examples of this development. We find here an attempt to analyse what may be called the natural life histories and life cycles of its basic constituents—parents, children, and kin.

4. Studies of a great variety of customs and institutional arrangements in terms of their relation to various aspects of social structure, and to the functioning of the major natural groups of the society as well as of the society as a whole. Examples of this approach include the analysis of joking relations and of the "mother's brother" (Radcliffe-Brown 1952; Goody 1959); and the studies of witchcraft, blood-brotherhood (Evans-Pritchard 1933; Eisenstadt 1956), magical hair (Leach 1958), and many other types of institutional devices or customs. These studies have often taught us to look beyond the manifest content of these usages, and have sharpened our understanding of the "meaning" of various customs and types of social and human interaction, such as commensality or even sexual relations, in terms of primordial images and sacred symbols.

5. Various comparative studies and analyses which have developed within the fold of social anthropological studies, the best examples of which are *African Political Systems* (Fortes and Evans-Pritchard 1940), *African Systems of Kinship and Marriage* (Radcliffe-Brown and Forde 1950), *African Worlds* (Forde 1954), and *Tribes Without Rulers* (Middleton and Tait 1958).

The societies studied by social anthropologists evinced great variation in the exact structure of different natural groups and institutions—territorial, kinship, and age groups; and various types of associations or political groupings. But in spite of these variations, anthropologists found that the close interrelationship between individual behavior, group structure, and institutional norms was always maintained by the mechanisms outlined above. It is true that anthropologists and sociologists are now beginning to doubt whether all of the social organization of these societies can be explained in such terms. Recently, it has been claimed that the absence of historical records in such societies and the two or three years that most anthropologists spent in the field "blinded" them to possibilities of change, of differentiation and variability of individual behavior, and of development of new group structures. It has also been claimed that the theoretical nature of their starting point may have biased anthropologists to emphasize those situations in which behavior was regulated by the mechanisms postulated by their model. Some of the most recent developments in social anthropology itself—for instance, Turner's (1957) study of Ndembu village life—illustrate certain of these shortcomings. (Additional criticism can be found in Fortes

1949a; Gellner 1958 and Gluckman 1959b give a fuller analysis.) Without doubt, however, this model contributed to the analysis of the functioning of at least some aspects of the societies studied.

IV

Armed with these basic concepts and approaches, social anthropologists began to study other types of societies, both historical and contemporary (modern), applying their approach and the assumptions implicit in their model to understanding the working of these societies. These studies can, in principle, be divided into several distinct types or categories, broadly similar to those outlined above for more characteristic anthropological studies.

A. The first category comprises studies of ecological communities—mostly of peasant villages and other units that are similar to, but not identical with, those in tribal societies. The most important of these studies are Arensberg and Kimball (1948) on the Irish peasant; Frankenberg (1956) on football and politics in a Welsh village under the impact of industrialization; Pitt-Rivers (1954) on the structure of an Andalusian village and its place in Spanish society; Barnes' (1954) study of a Norwegian parish; Srinivas' (1952) work on the Coorgs of India; and some of the Indian village studies (e.g. Marriott 1955).

B. The second category comprises studies of one main type of "natural" or institutional group in complex societies, usually examined in relation to the wider institutional setting. Naturally enough, the most frequently studied institutional unit is the family and kinship unit; but studies of such institutional settings as economic and political structure can also be found. Within this category may be distinguished three main types of studies, which overlap to some extent.

1. The first type of study, which deals with the internal structure of such groups, includes, Freedman's (1957) work on Chinese marriage in Singapore, and the studies of family and kinship structure in modern society by Young and Willmott (1957), Firth and associates (1956), and Bott (1957).

2. The second type comprises studies which emphasize the interrelationships between these groups and the wider institutional settings of their societies. The best examples are Firth's (1946) study of Malay peasant economy; Freedman's (1958) work on the Chinese lineage organization; Miller's (1954) "Caste and territory in Malabar"; Mayer's (1960) *Caste and Kinship in Central India*; Bailey's (1957) study of the interrelation between modern economy and caste in an Indian village; and Gullick's work on Malayan political structure (1958). These studies aim at understanding the operation of groups with which anthropologists were traditionally concerned—the peasant village, the lineage, the caste, or kinship unit—in more complex (but not yet modern) societies: to what extent they maintain their corporate identity; what functions such groups perform for their members in relation to the wider society; how the more general political or economic forces impinge on these units, as well as how these units are

integrated within" this wider institutional structure.

3. There are other studies which concentrate on the analysis of a specific form of "natural group" or institution within a complex society, and attempt to explain its relation to various aspects of the broader social structure. Smith's (1956) study of the Negro family in British Guiana, which attempts to explain the general social conditions that facilitate the development and maintenance of the matri-focal system of domestic relations and household grouping in the Caribbean, is the most important work of this type.

It is worthwhile to note an interesting convergence of the social anthropological studies discussed here with the somewhat different anthropological tradition represented by Redfield's studies (1955, 1956) of the "Great" and "Little" Tradition, and the mechanisms of transmission from the former to the latter. These studies were most explicitly focused on an analysis of the mechanisms connecting such "closed" units as village, family, or caste with the more central institutional system of relatively complex societies.

C. The third category comprises studies which attempt to analyse the "total" structure of relatively complex, but not modern, societies. The outstanding examples are Nadel's (1942) study of the Nupe, and Leach's (1954) *Political Systems of Highland Burma*. Although Leach's work, as its title indicates, deals mainly with political institutions, it is really concerned with the analysis of the concept of "total" society so often used by social anthropologists.

A special category of anthropological studies of "complex" situations, which cuts across the classification used above, includes those dealing with tribal groups or sub-units in situations of change under the impact of modern conditions. Examples are Gluckman's (1958) "Analysis of a social situation in modern Zululand"; Epstein's work (1958) on urban politics in Africa; Little's (1955) study of voluntary associations; Mitchell's (1956) description of the development of a particular dance among urbanized Africans; and Watson's (1958) study of the impact of a money economy on two African tribes. Stemming from earlier research on "detribalization," these studies have gradually focused on understanding the impact of processes of modernization on tribal structure, and the reorganization of this structure in the new situations. They have also concentrated on the ways in which new types of groupings, such as urban association trade unions, tend to crystallize in these new settings.

In addition to these, we may also mention various studies in other fields—especially industrial sociology (Burns 1955; Bradney 1957*a,b*; Gluckman 1956)—which deal with the nature of the personal and intergroup relations operative in modern industrial settings. These were influenced by some of the approaches to social anthropology, even if they do not apply the social-anthropological "model" in the same consistent way as those cited above.

V

The ultimate aim of these studies is to show how social behavior in complex societies is also determined by the groups and institutions in which individuals

participate, and how such behavior contributes to the functioning of different groups in the society and of the society as a whole. But the facts which they have to explain, and the problems they have to analyse, differ somewhat from those which are usually dealt with in anthropological studies. It is true that most of these studies deal with types of groups or institutional units which are, to some extent, similar to those studied in tribal societies; yet these units are not as self-contained or closely overlapping as in the latter. In the complex societies, the units interlock into wider settings in ways different from those of primitive societies. It is this interlocking that creates many problems for the understanding of patterns of social behavior and their relation to group and institutional structure in these societies.

Thus Barnes, in his study of the Norwegian parish (1954), faced not only the problem of explaining the ways in which the members of the parish are interrelated, or in which different committees in the parish work, and how they are related to the kinship, working, and class structure *within* the parish. He had also to deal, first, with the problem and the possibility that members of the same general social category will be able to choose different roles and participate in different associations; second, with the development of new groups and types of activities under the direct impact of outside forces; and third, with the relations of all these groups to whatever may be called the over-all Norwegian society.

Pitt-Rivers (1954) was dealing not only with the internal structure of his Andalusian village but also with the problem of *differential* impact of the central forces on the activities of different individuals, belonging perhaps to similar groups in the village.

In addition to investigating internal village politics, the importance of football and various ceremonies in the framework of these politics, and their impact on the unity of the village, Frankenberg (1956) is also concerned, even if indirectly, with the impact of wider forces, such as those of industrialization, on the internal cohesion and continuity of the village.

In his study of Chinese lineage organization, Freedman (1958) is interested, not only in the internal structure of the lineages, but also in the ways these interacted with the over-all political structure and with the imperial bureaucracy; how they were able to adjust themselves to this bureaucratic, non-kinship organization; and what functions they performed within that wider context.

In his work on Chinese marriage and kinship in Singapore (1957), Freedman is dealing not only with the internal structure of the major kinship and marriage groups as relatively "closed" units, but also with the problem of the breaking up of these corporate units, and of the influence of the "anonymity" of Singapore city life on marriage and kinship behavior, on these units, and on family and kinship relations.

Nadel (1942) was the most explicit in focusing his interest on the over-all working of a complex society without concentrating exclusively on any small sub-unit. Throughout his study of the Nupe, he was interested in describing and analysing how different local groups and different types of ordered social activities—

economic, ritual, educational, and political—are related to the working of the over-all Nupe society, and how they create the specific Nupe identity.

Little (1955), Epstein (1958), Banton (1957), Mitchell (1956), and Watson (1958) deal not only with the processes of change and disintegration of tribal units, but also with the ways in which these groups are transformed in a new setting—how entirely new groupings are formed; how conflicts that do not necessarily contribute to the equilibrium of the encompassing society may develop between various types of groups; and how these groups compete for the allegiance of the same persons, or influence their behavior in contradictory ways. Thus, they also had to explain the ways in which the people they studied were drawn into social frameworks and influenced by social forces of processes transcending the major natural ecological groups (e.g. wider occupational, economic, or political forces) and how the behavior of people in these groups, and their choices of roles, are related not only to their immediate group allegiances, but also to wider social forces, some of which did not originate within the “old setting.”

VI

Thus, the problems which were either implicit or explicit in the data presented by social-anthropological studies of complex societies are of a somewhat different order from those involved in the study of tribal societies.

The main difference could be found in the fact that patterns of social behavior in these complex societies were influenced and regulated by many forces and factors which were not embedded in the structure of the major social groups or their interrelations (e.g. they were not regulated by the interrelations between domestic, kinship, and local groups), and which were not necessarily borne out by people interlocked in the same situations and groups.

This difference has, necessarily, posed problems of the nature of the mechanisms regulating the relations between various types of social behavior, group structure, and the wider institutional structure of a society. How, then, did the social anthropologists approach these problems and what were their contributions towards their solution?

It is here that the most important contributions of these studies can be found. These contributions were primarily made through the discovery of several distinct ways in which the regulative mechanisms found in tribal societies operate in more complex societies as well; and the discovery that in the latter societies such mechanisms are not necessarily always embedded in concrete groups.

A. These studies discovered and analysed various areas of life in complex societies in which very close interrelations exist between people participating in different situations and groups, and these interrelations regulate their social behavior in different groups. The importance of this type of mechanism in corporate groups and institutional fields within complex societies is best exemplified by Freedman's *Lineage Organization in South-eastern China* (1958) and Miller's work on caste in Malabar (1954). Young's (1957), Firth's and as-

sociates' (1956), and Bott's (1957) studies of family and kinship in London, although more limited in scope, also show the importance of these close personal interrelationships in the structure of at least some corporate groups and institutional settings of modern societies. Such close relations were also often found to be operative in many informal social relations not organized in corporate groups or institutional frameworks.

Barnes (1959), in his review of Frankenberg, has aptly summarized the importance of this mechanism in some ecological units of modern societies:

In Pentrediwaith the same core of individuals interact with one another in many different situations, and their behavior in one context is influenced by their commitments in many others. This is true of almost all communities in the primitive world, and it is a necessary condition for using the slow and indirect field techniques specific to social anthropology (cf. my [i.e. Barnes'] "Class and Committees in a Norwegian Island Parish," *Human Relations*, Vol. VII, 1954: 44). The ethnographer, with his traditional distrust of direct questions and questionnaires, and his desire to do more than test a bald hypothesis or establish a correlation, is particularly well qualified to observe these lengthy and devious sequences of social action and to analyze them in sociological terms. As social anthropologists we have long held, either as an article of faith or a matter of academic political expediency, that we have valuable contributions to make in the study of advanced societies, but it has not always been clear, either to us or to our colleagues in other disciplines, precisely what these special contributions are. At one time the answer might have been "community studies." But some of these, although informative, appear not to differ greatly from the studies carried out by geographers and other social scientists with background and objectives different from those of social-anthropologists; other studies under this rubric are merely social surveys of populations which may or may not form a community. Frankenberg's pioneering study of Pentrediwaith shows that one useful contribution which social anthropology can make to the investigation of advanced societies is the observation and analysis of politics round the village pump.

B. The anthropological studies cited here have also found that direct relations between primordial symbols and values and various social relations are operative in several areas of complex societies. Such direct relations have been found, first, as Young's and Willmott's (1957) studies have especially shown, in the field of family and kinship relations. Second, anthropologists have found such relations to be very important in an individual's attitude towards his community or society. Third, they have also been found to be operative in more diffuse social areas and relations. (For a more general evaluation of the place of primordial images in social organization see Shils 1957.)

Thus, various studies (Burns 1955; Bradney 1957a,b; Lupton and Cunnison 1957) have shown that in many areas of life in complex modern societies there develop, between persons participating in some common formal frameworks, different types of relations, which are regulated either by their interrelations in other situations or by such primordial images and symbols as "manhood," "human trust," and "age," or sharing of common "fate," "neighborhood," or "goodness." Such

relations and symbols have been found to cut across hierarchical relations—or any other formal relations and groups—and also to influence patterns of friendship and diffuse personal relations. Such relations may often be more forceful than the formal role-definitions, and may greatly influence the performance of these formal roles. Similarly, some of these studies indicate that different usages—such as commensuality, certain types of friendship, name-giving, gift-giving, plays and matches—in many of the complex and even modern societies, are often imbued with various primordial meanings and values (Gluckman 1959a). These studies, for instance, deal with the ways in which many conflicts develop between the European industrial forces and the more traditional tribal forces, and how some continuous—although not necessarily stable—interrelations develop between these opposing forces.

While some results of these studies may seem identical with those of various researchers on “primary groups,” in fact they often go beyond the latter; or at least complement them. Most research on primary groups has pointed out mainly the importance, within formal organizations, of so-called solidary and face-to-face relationships. Social-anthropological studies, however, have gone—potentially at least—beyond these findings. They show how these formal frameworks not only are imbued with solidary relations, but are also greatly influenced by the same individual’s *personal* commitment in other settings and situations, and are invested with certain primordial meanings which are prior to the formal role-definition as well as to the various solidary relations, and may even, to some extent, regulate them.

C. Several of those studies attempt to apply some of the more general principles of social organization, and especially of the interlocking of potentially conflicting forces in one “total field,” to more complex situations or societies. Thus, several studies of “colonial” situations in Africa, or of certain aspects of industrial structure (Gluckman 1956, 1958), have sharpened our awareness of how conflicting forces may sometimes become interrelated in a single “total field”; and of how different concrete patterns of behavior in such a field can be explained in terms of the relative strength of these conflicting forces within their common framework, and of the functioning of this framework.

Significantly, of all the regulative mechanisms found in the social-anthropological model, only one—that of the coexistence of different types (jural, political, economic, ritual, etc.) of social activities in the same groups or situation—was not found to be of great importance in the complex societies.

VII

The studies which have shown how various mechanisms operate in complex societies have also contributed, explicitly or implicitly, to the analysis of two other aspects of these societies.

First, the delineation of the areas and relations in complex societies in which the types of relations postulated by the model of social anthropology still persist points more sharply to those structural points at which these cease to operate and are replaced by more general and differentiated mechanisms of ordering social ac-

tivities—even if, as we shall see, specific anthropological concepts do not help very much in the analysis of these mechanisms.

To give only one example, we may quote Freedman on some of the problems of Chinese marriage in Singapore (1957):

Marriage in Singapore no longer represents a link between two corporate groups, and Chinese in the Colony, while preferring to seek their brides within their own dialect-groups (and often within narrower divisions of these groups), treat a new marriage as a matter concerning only the bridal couple and their fellow household-members. Members of clan associations may be invited to wedding celebrations but they have little direct interest in the marriage formed. The structure of Singapore Chinese society makes for the individualization of marriage. This process is also sanctioned by modernist ideas which derive their potency from their connection with twentieth century nationalism.

In the disruption of marriage the inadequacy of Chinese mechanisms to control domestic disputes is very apparent. Since there are no clearly defined larger kinship units, no local units in urban life, and no formal structure of authority in the rural settlements, marital quarrels tend frequently to find their way to persons and bodies standing outside the limits of Chinese society itself. It is at this point that “government,” in the shape of the magistrates’ courts and the Department of Social Welfare intervene decisively in Chinese life. Divorce among Chinese in the Colony is essentially a matter of mutual agreement between the spouses. However, just as in the formation of marriage the anonymity of Singapore life allows uncertainties in the status of men and women as husbands and wives, so in the disruption of marriage there are sometimes ambiguities which arise when wives leave their husbands (or are abandoned by them) and “follow,” as the Chinese say, “other men.”

Among studies of a somewhat different anthropological tradition which bear on this problem, we should again mention Redfield’s (1955, 1956) work on peasant societies. He has described the differentiation of the different orders of social relations in the more complex societies, but at the same time has given us a feeling for the interrelation of these different orders—even if he did not explain this analytically.

Second, these studies can contribute much to the development of comparative studies. This can best be seen in Smith’s work on the West Indian Negro family (1956). Smith attempts to give an exact specification of those broader mechanisms of societal differentiation which impinge on and influence the structure and functioning of family organization. Thus, he argues that the husband’s role in the matrifocal family is strongly correlated with the role of men in the economic system and in the system of social stratification in Guianese society. This specification, when fully worked out, may serve as a starting point for further comparative work on the family, and for parallel attempts in other fields (Eisenstadt 1957). Similarly, Watson’s (1958) analysis of the impact of a money economy on tribal society contains very interesting hypotheses about the influence of different structural variables (e.g. patrilineal and matrilineal descent, patrilocal and matrilocal) on the process of adaptation of modern forces within this tribal framework, and in terms of the maintenance of this framework.

Finally, these studies, especially those related to problems of change, have necessarily called attention to certain problems of the processes of transition from relatively homogeneous societies to more differentiated social orders, and have pointed out some of the problems and patterns of behavior and structure which develop in such a process.

VIII

The significance of all these contributions goes beyond their specific analyses of the areas enumerated above. Their main significance for sociological analysis lies in their enlarging our understanding of the complexity and variety of the social mechanisms which regulate social behavior and organize the division of labor and the interrelations between different groups in so-called complex societies. Sociological and anthropological analysis has often tended to classify societies into "types" in terms of some such major mechanisms. Mechanical and organic solidarity, *Gemeinschaft* and *Gesellschaft*, "sacred" and "secular" societies, "folk societies," and often the very terms "primitive" and "complex" societies, have been used to denote these types. And although it has become almost a commonplace of sociological and anthropological literature that "pure types" of such societies do not exist, sociological and anthropological analysis seldom attempts to delve too deeply into the implications of this assertion for the analysis of concrete ways of social organization. It is here that the social-anthropological studies of complex societies have made their most distinct contribution to sociological theory. They have clearly indicated how regulative mechanisms, belonging as it were to one type of society, operate in some parts of other types of societies and constitute basic components of social organizations. Moreover, they have shown that in the complex societies these regulative mechanisms do not operate in closed, watertight compartments, but instead very often interpenetrate with other mechanisms, permeate the same areas of social life as similar mechanisms, and are not necessarily always embedded in the structure of the major groups of a society. In this way, these studies have contributed to the reassessment of the sociological image of modern societies. They have shown, at least by implication, that modern society is not a "mechanized" one, in which atomized individuals live in separation, ruled only by impersonal forces. Rather, various closely interwoven personal and group relations, on the one hand, and relations permeated with symbolic and primordial meanings, on the other hand, constitute basic components of even the most differentiated type of society.

Of special significance from this point of view is the implication of these works for the analysis of the place of primordial symbols and ritual values in the organization of a society. The importance of "ritual" or religious activities in primitive societies has often been explained mainly in terms of little social differentiation, and of the intensity and closeness of social interaction. But anthropological studies of complex societies (as well as several other studies) indicate that the significance of primordial images and of rates of interaction and social differentiation may vary independently; that symbolical primordial images and values cannot

be reduced to such rates of interaction; and that they constitute an independent realm which is autonomous and of great importance in any society, even if the ways in which they operate differ from society to society.

IX

The important contributions of social-anthropological studies of complex societies do not, however, necessarily imply that they have solved all the problems which were implicit—and also, very often, explicit—in their materials.

While these studies pointed out some of those areas in the social structure of complex societies in which certain regulative mechanisms most predominant in primitive societies were operative, they were less successful in analysing areas of social structure—which are still of predominant importance in most of these complex societies—within which such mechanisms do not operate.

As we have seen earlier, their data show the existence, in these societies, of many aspects of social behavior which could not be explained solely in terms of place in a given group or institution, or in terms of contribution to the functioning of these groups in the total society. These data have also indicated that in most complex societies, the different types of social activities—ritual, economic, contractual, political, etc.—are not closely interwoven and interrelated in the same situations or groups. In complex societies, each of these types of activities often tends to crystallize into specific institutional frameworks or orders of its own. Therefore it was more difficult to point out the ultimate ritual or primordial sanctions of many contractual types of behavior.

1. In more concrete terms, these studies and the concepts employed in them did not explain how behavior was regulated in situations in which individuals could choose between different roles and groups, and in which different and contradictory institutional forces and groups (e.g. tribal and family loyalties, and modern political or occupational groupings) impinge on the individuals participating in them, and may often have made different—even contradictory—demands.

2. The concepts could not explain the ways in which many patterns of individual behavior and activity (e.g. in leisure time activities, in work-place, and in family groups) were not entirely bound to concrete groups or embedded in them, but were widely dispersed among different and seemingly unconnected situations.

3. They did not explain how new patterns of behavior, groups, and situations (e.g. modern working situation, trade organization, political affiliation) were continuously emerging, crystallizing, and impinging on different individuals and making different, often conflicting, demands.

4. They did not explain the ways in which different types of social activities—contractual, juridical, political, ritual, etc.—are organized in different "orders" within a society, and how all these orders impinge on and regulate various situations, groups, and patterns of behavior within the society.

5. They did not explain how—if at all—many conflicts between different groups in these societies (e.g. political and economic conflicts between Europeans and Africans cutting across any such society) were regulated, or how they could be said simply or outrightly to contribute to the continuous integration of an ongoing society. This also seems to apply to the analysis of the function of any one group within the “total” society. These studies have shown that, in the more complex societies, it was more difficult to delineate the exact function of each such group within the “total” social structure, or the exact functions which any such groups performed in relation to the basic institutional spheres (e.g. economic or political) of the society. Hence, it seems that the whole concept and reality of the “total” society is here much more problematic than in the study of primitive societies; and that this “total” society is no longer a relatively well-circumscribed group with clear ecological, personal and cultural boundaries.³

Thus, most anthropological analyses of complex societies were unable to analyse fully the problems of a complex division of labor in society. The only partial exception is, again, Nadel's work on the Nupe (1942). His descriptions of the political, ritual, and economic life of different groups within the Nupe State are intended not only to present an internal analysis of these groups, but also to show how different types of activities (e.g. economic, political, ritual) are organized in the various orders of mechanisms which affect the “whole” of Nupe society. Nadel clearly distinguishes purely economic, semi-market activities or ecological arrangements from such organizations as age groups and various ritual-symbolical activities (e.g. the Ganni ritual; Nadel 1949), which serve as mechanisms for the transmission of sacred common symbols to different sub-groups in the society. While he shows how even economic or political activities and mechanisms are directed and influenced by “cultural” or ritual values, he recognizes that these are not always embedded in the structure of the different groups, but rather work through different, more complicated, mechanisms. He points out some of the ways in which they seem to operate, even though he also does not fully analyse how each of these social orders and mechanisms is organized, or how they operate together in a relatively complex society.

X

The difficulties met by social-anthropologists in explaining all the problems of the division of labor in complex societies can most clearly be seen, perhaps, through an analysis of the concepts they employed and coined for the study of the complex societies, once they found that their traditional techniques and concepts did not suffice to deal with all the problems inherent in the material. The most important concepts evolved for these purposes are “network,” developed by Barnes (1954) (and also used by Bott 1957); “social field,” initially developed by Fortes and by Gluckman and his

students (1958); and “social organization,” as developed by Firth (1951).

The term *network* is used by Barnes (1954: 43) in the following way to describe the complexity of the social relation to be found in Bremnes:

Each person is, as it were, in touch with a number of other people some of whom are directly in touch with each other and some of whom are not. Similarly each person has a number of friends, and these friends have their own friends; some of any one person's friends know each other, others do not. I find it convenient to talk of a social field of this kind as a network. The image I have is of a set of points some of which are joined by lines. The points of the image are people, or sometimes groups, and the lines indicate which people interact with each other. We can of course think of the whole of social life as generating a network of this kind. For our present purposes, however, I want to consider, roughly speaking, that part of the total network that is left behind when we remove the groupings and chains of interaction which belong strictly to the territory and industrial systems.

The concept denoted by the terms *social field* (which was also used, although in a different way, by K. Levin and in sociometry) and *social situation* has been developed by Fortes (1949b) and then used by Barnes (1954), Gluckman (1958), and Epstein (1958) to deal with the new type of total society which they found, and with the interrelations between its parts. Barnes (1954: 42–44) uses this concept in the following way:

Thus in terms of this analysis we can isolate three regions or fields in the social systems of Bremnes. Firstly there is the territorially-based social field, with a large number of enduring administrative units, arranged hierarchically, one with another. . . .

The second social field is that generated by the industrial system. Here we have a large number of interdependent, yet formally autonomous units such as fishing vessels, marketing cooperatives, and herring-oil factories, connected with each other functionally rather than hierarchically, yet each organized internally in a hierarchy of command. . . .

The third social field has no units or boundaries; it has no coordinating organization. It is made up of the ties of friendship and acquaintance which everyone growing up in Bremnes society partly inherits and largely builds up for himself. Some of the ties are between kinsmen. A few of them are between people who are not equals, as between a man and a former employer with whom he has kept contact. Most of the ties are, however, between persons who accord approximately equal status to one another, and it is these ties which, I think, may be said to constitute the class of system of Bremnes. The elements of this social field are not fixed, for new ties are continually being formed and old links are broken or put into indefinite cold storage.

A somewhat different definition is given by Epstein (1958: 234):

“The Copperbelt,” then, has to be seen as a single field of social relations which is composed of different sets of relations, each of which forms a distinct sub-system. Fundamental to this social system is the dominant cleavage between Europeans and Africans, and this cleavage influences behavior and institutional growth within each part of the social field. At the same time, each sub-system enjoys a certain measure of autonomy; they do not react in the same way and at the same time towards the external stimuli making for social change.

The term *social organization* has been used by Firth

³ It is significant to note that some doubts about the validity of the concept of “social structure” or total society were pointed out by Leach (1954) in his study of the Burmese highlands.

to denote the difference between the "formal" or normative social structure and the patterns of individual behavior which develop within that structure.

These concepts, and especially that of social network, are very useful in that first, they describe some of the more complex interrelationships which tend to develop between the more closed "homogeneous" groups and the wider social setting; and second, they may make us sensitive to the complex nature of the forces which operate within any single unit of behavior or group within these groups and societies. But, with the partial exception of "network," they do not help greatly in the systematic analysis of the great variety of problems of social division of labor, which are explicit or implicit in the material presented in social-anthropological studies of complex societies.

Thus, while the concept of social field (or fields) and sub-system *does* tell us that we have to look for some general divisions of society and their interrelations (economic, political, etc.), it does not help us to find explanations of the principles of such interrelationship, or of the ways in which different groups and social situations are interrelated with respect to these divisions. It does not help in the systematic analysis of the ways in which these divisions impinge in different ways on different individuals, or explain why some individuals are drawn more than others into a given field. Nor do the uses of this concept explain the ways in which possible conflicts between such fields develop and are, or are not, resolved.

Similarly, while the concept of social organization does indicate clearly the existence of the problem of differences between individual behavior and institutional structure, it does not, as yet at least, show us what are the major components of this social organization, how they vary, and in what ways they may affect different aspects of social structure.

It is perhaps only the concept of network that to some extent provides a potentially new analytical tool. It clearly describes or points out the existence of some differential interrelation between different people who are not organized in corporate groups; and it may help in the analysis of the relation of different persons, acting in such a network, to different types of social roles and institutional frameworks. In this way, the concept of network does at least point out one way—beyond embedding in the structure of concrete groups—in which the various regulative mechanisms can be organized. However, although Bott has analysed the relations between certain types of networks and division of labor (1957), this concept has not yet been used to explain how different people are drawn into different networks, or how such networks develop. Moreover, the concept is still limited to that type of social relation in which some of the close interrelationship exists between the same people in different situations.

Thus it may be said that, although these concepts describe important aspects of social organization and structure in these societies, most of them (with the partial exception of network) do not help in an analysis of the systematic variations of behavior in those types of situations in which close interrelations between individual behavior, group structure and institutional

order do not exist.⁴ These concepts could not be applied to the analysis of different mechanisms or regulating social behavior, or of social division of labor, which were not embedded in the structure of concrete groups; or to the problems inherent in the coexistence and development of different orders of social relations in the same society.

From this point of view it is interesting to note two facts:

1. In so far as anthropologists deal with these different institutional orders, they tend to employ the usual sociological nomenclature, usually without attempting any critical appraisal of it.

2. It is very significant, from this point of view, that most of these anthropological studies (with the partial exception of Epstein's) have not made *systematic* use of the concept of "role"—nor have they critically appraised it. The importance of this concept, from the point of view of our analysis, is that it is the basic sociological concept (even if one of the first to coin it was an anthropologist, Linton) which attempts to link together individual behavior and its social function, not only on the level of concrete groups. Its aim is to show how the behavior of individuals in different positions in the society is influenced, not only by membership in concrete groups, but also by various broader institutional settings and forces, each of which attempts to define certain aspects of a social position; to make (very often new) demands on its incumbents; and to employ different sanctions to ensure their acceptance of such demands.

The limitations of these analyses are perhaps most evident when they deal with the problems of change. Social-anthropological studies dealing with this problem do not usually explain how new frameworks of social organization—new "social fields"—have emerged out of the older ones, or how the new institutional order and norms that develop have become crystallized.⁵ Nor do they systematically explain the forces which influence different individuals to choose between alternatives in the new situation. Rather, most of these studies take the existence of some of these new frameworks for granted. Starting off from this premise, they tend to investigate the different groupings that exist within them.

It is very interesting to compare, from this point of view, Watson's analysis of the impact of money economy on the Mambwe with other studies of change, for instance that by Epstein. Watson's main topic focused on the continuous functioning of the tribal groups, and how money economy did *not* destroy tribal cohesion. Hence, his analysis is much more akin to the usual anthropological studies and gives a much more "complete" picture of a still relatively undifferentiated society than does Epstein's analyses of more complex situations, which necessarily focused on individual behavior

⁴ The criticism levelled against the Warner school's application of social anthropology stressed this point in a pertinent way (Goldschmidt 1950).

⁵ A possible partial exception to this is Worsley's (1957) study of cargo-cults. But, significantly, this study uses specific anthropological concepts to a smaller degree.

and the structure of groups in a given situation, taking the development of the situation more or less for granted.

XI

We see, thus, that social-anthropological studies have made a distinct contribution to the analysis and understanding of complex societies, and have provided new perspectives for such analyses. On the other hand, they were not very successful in analysing *all* the different aspects of social organization in these societies, and especially those problems of the complex division of labor which were related to the co-existence of different types or orders of social activities, or to the simultaneous operation of different types of regulative mechanisms within a given society.

While the most significant general contribution of these studies was their showing systematically that within any "type" of social structure there operate varied, different types of regulative mechanisms, they did not go on to analyse the general theoretical implication of this finding: *how* these different types of regulative mechanisms operate in the same society, and what are the relations between them. It was exactly at this point that their analysis stopped; and they have concentrated mainly on the operation of one (broad) type of such mechanisms within complex and modern societies.

However, a further exploration of this implication, and of the general problems arising from the anthro-

pological study of complex societies, may contribute much to general sociological theory, and may also help reassess the analysis of tribal societies. Several recent studies of simple societies, such as Turner's (1957), as well as some of Leach's queries about the anthropological definition of social structure (Leach 1954; Gellner 1958)—not to mention Lévi-Strauss' analyses of different orders of social life (1953, 1959)—have pointed out that, in at least some areas of social life in primitive societies, there operate types of regulative mechanisms that are more varied than has been postulated by the social-anthropological model; and that in these societies even these mechanisms are not always embedded in concrete groups—even if the degree of differentiation between them is less than in the more complex societies.

The further exploration of the insights gained through analysis of the limitations encountered in applying the model of social anthropology to complex societies may stimulate such reassessments of simple societies by looking for areas in their social structure where more differentiated regulative mechanisms of social behavior operate side by side with those mechanisms which are embedded in the structure of concrete groups. In this way, as well as through sharper confrontations between the continuously developing anthropological and sociological studies of complex societies, the full implications of these studies for sociological theory and analysis may be brought out.

Comments

By J. A. BARNES and A. L. EPSTEIN*

Eisenstadt's lucid and stimulating summary evokes some comments on matters of judgment, others on matters of principle. For example, he judges "social field" to be a concept evolved to grapple with complex societies. Yet an early application of this notion, in the guise of the "structure of the situation," appeared in Fortes' 1937 account of fishing magic among the Tallensi, where he describes a situation satisfying all the criteria for non-complex tribal conditions set out in section II of Eisenstadt's article.

But Eisenstadt is not trying to write a definitive history of recent social anthropology, and such comments are not important. We would not accept the anthropologist-sociologist dichotomy that he implies, nor the picture of British social anthropologists as a brave band of brothers setting out to tackle all sociological problems in glorious ignorance of the work going on in other countries and in cognate disciplines; but this also is marginal to his argument.

A more serious matter of principle is the dichotomy between description and analysis that Eisenstadt makes, particu-

larly in section X. He asserts that the concept "social field" can be used to describe complex interrelationships, but not to analyse and explain the way in which different groups and social situations are interrelated. What does this mean? For example, Fortes makes use of the social field concept in his discussion of the spread of matrilineal kin ties (1949b: 286ff.). Is this discussion description or analysis? Surely it is both, and it is a mistake to think that the two processes can be separated in practice. Indeed we would argue that description is "merely" analysis using yesterday's categories, just as analysis is "merely" description in terms of tomorrow's categories.

We agree that the concept of a social field containing subsystems does not in itself help to explain the principles of division within that field, or the interrelations between them. We would be very much surprised if it did. Rather, we would assert that the adoption of the "field" concept in the study of complex societies, where so much of social behaviour is compartmentalized, is a necessary first step towards the formulation of some of those very problems that Eisenstadt raises. Their solution, it seems to us, calls, not for an attack on the primary concept of the social field, but for the development of a

body of auxiliary analytical tools, of which "network" might be one. One of us has made tentative use of the notion of "feed-back" for exploring the interrelations of the subsystems within the social field, though it is clear that this still needs much more careful working-out (Epstein 1958: 237; cf. Mitchell 1960).

Eisenstadt's antithetical treatment of description and analysis appears to have deflected him from tackling an important and as yet unresolved issue that arises from the study of complex societies. For Radcliffe-Brown, the goal of social anthropology was the discovery of social laws. One of the reasons he remained lukewarm towards plans for tackling the study of complex societies was his expectation that the regularities in social life which he believed to exist in tribal society would, of necessity, be absent in complex societies, or at least would be much harder to uncover. Despite Radcliffe-Brown's assertion that few, if any, social laws have yet been established, there are many social anthropologists who still hope to find them, even in complex societies. Others of us are content to describe social life as perceptively as we can, without worrying too much about the discovery of social laws (cf. Barnes 1958: 65-67). Eisenstadt continually refers to our pro-

fessional task as "understanding" social phenomena; but although he seems to imply that this entails something more than "describing," it is never clear to us what "understanding" means in operational terms. He holds, we infer, that it is better to analyse than to describe, and tries to help us over this hurdle. But he leaves himself stuck on the fence dividing description from social laws, sitting on a soft cushion of understanding.

We agree with Eisenstadt that, in dealing with situations of social change, most of us have taken for granted the existence of "new networks of organization," particularly when these have been such things as a colonial administration, a mining company, or a Christian mission. But this assumption has often been made quite explicitly, to delimit a manageable area of inquiry, and not because these impinging frameworks have been thought to be beyond scrutiny. Recently, one of us (Barnes 1960) made a preliminary sally in the field of politics to bring some of these phenomena into our analysis.

By J. H. M. BEATTIE*

Eisenstadt's survey is both able and timely. My only comments concern certain aspects of the theoretical framework which he sets out, very competently, in the first two or three sections of his paper.

(1) I feel that Eisenstadt's formulation slightly tends to overstate the distinction between social behaviour and the various norms which are said to be operative in the social structure. I think that some social anthropologists would hold that norms, values, and beliefs (between which Eisenstadt does not distinguish) are better regarded as aspects of, rather than as related to, social behaviour, so that no meaningful account of the latter can be given without reference to the former. The tendency to reify this analytical distinction leads to the formulation of the interests of modern social anthropology in such terms as "studies of the ways in which major norms . . . are upheld by individuals," a formulation which, with its suggestion of a population striving to conform to a set of norms apprehended explicitly, somewhat misrepresents the actual concern of fieldworkers, to whom, for the most part, norms are not given as data, but are rather inferred through the analysis of social (including verbal) behaviour itself.

(2) Eisenstadt's characterization of modern British social anthropology as concerned with the explanation of—for example—the economic or political activities of social groups in terms of the "needs" of these groups and of the so-

ciety as a whole has a curiously old-fashioned ring. Of course people, both individually and as groups, have needs (though it is less plain, *pace* Durkheim and Radcliffe-Brown, in precisely what sense societies have them); but I think that few modern social anthropologists attempt to explain social rules or norms by reference to such needs. Thus, one does not seek to explain a particular marriage rule, for example, in terms of sexual need, or the need to procreate; these needs are surely taken for granted.

Explanation of such a rule is sought rather in terms of its significance for the person who acknowledges it, its implications for other norms and institutions in the society, and (where there is evidence) its history. Explanation in terms of some non-biological "social" need—for example, the continuance of society itself—is hardly more satisfactory, and often amounts to little more than the assertion of what is already sufficiently obvious: that if a particular norm is quite compatible with other institutionalized norms in the society, both it and they are likely, all other things being equal, to endure.

(3) This leads to the third general point on which I should venture some mildly critical comment: Eisenstadt's assertion that most modern anthropological studies explain patterns of social behavior through analysis of "total" societies. It has been pointed out often enough in recent years (e.g. by Popper in *The Poverty of Historicism*) that a total society is not a datum for analysis; what social anthropologists analyse are social institutions, and they attempt to demonstrate functional relationships between these and other social institutions, as Eisenstadt very ably shows in his paper. What they cannot do is to relate them to any such entity as a "total" society, for there is no such entity to relate them to.

I am sure that Eisenstadt is well aware of the importance of these considerations and their implications—indeed, the development of his analysis shows that he is. In fact, I think that my major criticisms would concern certain of the terms in which he sets out the theoretical framework for his study, rather than the framework itself. But these terms are, perhaps, quite important, especially for readers unfamiliar with recent work in these fields. Unless we are fairly rigorous, it is all too easy to slip back into the old dichotomy between "behaviour" and "ideas," into Malinowskian conjecturing about "needs" as affording an explanatory principle in social anthropology, and into the "holistic" fallacy that societies can somehow be appre-

hended as totalities and that such an apprehension is a valid analytic tool.

By MEYER FORTES*

Eisenstadt's admirable elucidation of the postulates and procedures followed, often only implicitly, by British social anthropologists is most instructive to a practitioner of the craft, like myself, whose instinct is to shy away from methodological discussions. His analysis is, in my judgment, correct as far as it goes, though it needs a minor adjustment of perspective in one regard. I do not think that our "models" and "mechanisms" are as systematically conceptualised or as deliberately applied as Eisenstadt's exposition suggests. They provide *directives* for field research and theoretical analysis, rather than *operational rules*. In Eisenstadt's terminology there are, I would suggest, at least two models that serve as such directives. In homage to their classical exponents, one might be called the Malinowski model and the other the Radcliffe-Brown model, though as we well know they also owe much to others, e.g. Durkheim and Mauss. The Malinowski model directs the investigator to begin from an empirical isolate of custom, e.g. the Kula, or the Chisingu (Richards 1956); the Radcliffe-Brown model directs him to begin by establishing an analytical or paradigmatic isolate of social structure—e.g. a lineage system—or of ideology (thought, belief, value, etc.)—e.g. a totemic system. Evans-Pritchard's *The Nuer* and my own *Dynamics of Clanship among the Tallensi* were based fairly explicitly on this model. But in most of the monographic studies referred to in the first three sections of Eisenstadt's discussion, the models are, to a greater or less degree, mixed; and, of course, some of the most significant ethnographic work of the past thirty years (e.g. Schapera's *Handbook of Tswana Law and Custom*), to which there is no allusion in Eisenstadt's article, is independent of either model.

However, if we consider only these two models, we can see that Eisenstadt's mechanism 1 (section II) has relevance only to the Malinowski model. An empirical isolate is a composite phenomenon, as Mauss showed in *The Gift*, and the essential unity amongst its "aspects"—to use Malinowski's term—lies in the fact that all can be seen as activities of the "same people." I do not see any difficulty other than that of scale in applying this model in a "complex" society. What has hitherto happened is that it has been misapplied. It cannot be used in the study of a village or a defined

geographical area, which is not an empirical isolate in the sense I have given to this term. But it could be applied in a study of the Christmas festival in England, for instance. This is a national festival exactly parallel to the Kula; it has economic, religious, political, philosophical, educational, etc. "functions" (sc. "aspects").

If I understand him correctly, Eisenstadt's mechanisms 2 and 3 are more relevant to the Radcliffe-Brown model. But I have a feeling that Eisenstadt has not identified the distinctive features of this model as clearly as he has perceived those of the Malinowski model. In terms of the Radcliffe-Brown model, there are no "family, kinship, or political activities" (my italics). For such concepts as "family," "kinship," and "political" denote arrangements of social relations, and these are manifested in all the "activities" that can take place in a society. They are analytical concepts, not referable to any one empirical isolate. In this model we are not concerned with the "same people," or with interrelations of activities, or of "culture" and "social relations." We are concerned, precisely and strictly, with the matrix of social relations which is analytically prior to—in a sense outside of—the values and norms referred to by Eisenstadt. If this were not the case we could not have such general, comparative categories as matrilineal descent or segmentary political system. The anthropological contribution has been to demonstrate in the simpler societies that the matrix of social relations constitutes a unitary and multidimensional system. Traditionally, the territorial, kinship, and political dimensions have received primary emphasis. What recent structural theory has investigated has been the interrelations of these dimensions, as in the studies by Turner and Smith cited by Eisenstadt. Two other important contributions to this development are A. W. Southall's *Ahur Society* (1954) and L. A. Fallers' *Bantu Bureaucracy* (1952).

This model, in its pure form, is of course difficult to apply in a complex society. But recent studies of caste (cf. Cambridge Papers in Social Anthropology, No. 2, 1960, edited by E. R. Leach) show that it is as applicable to Oriental civilisations as to the preliterate societies classically studied by anthropologists. That it is applicable also, given the resources and guidance, to industrial Western society was shown over twenty years ago in Lloyd Warner's series on Yankee City. The problem, as I see it, is not to look for "types or groups or institutional units . . . similar to those studied in tribal societies." Where this is done, there is danger of lapsing into triviality and anecdote. The task is,

quite simply, to see if and how the model can be applied. There may be a closer structural parallel between the trade union system of a European country and the corporate lineage system of an African state like Ashanti, than between the latter and the so-called "kinship network" made so much of in some current writings.

By JACK GOODY*

Eisenstadt's review raises a number of interesting issues. The point I wish to take up is incidental to his treatment of specific material, but my excuse is its importance, as I see it, for future developments.

Flattering as it is to British social anthropology to be singled out for such consideration, I have certain misgivings about framing a discussion solely around its achievements, especially in a field that overlaps so definitely with other social sciences, which share, to a considerable degree, the same theoretical background. In the present instance, for example, it would surely have been an advantage to include some consideration of such work as Whyte's *Street Corner Society* (1943), Davis and Gardner's *Deep South* (1941), as well as some of the other studies deriving from Warner's investigations in the U. S. A. But apart from this concentration on the work done in one country, it seems to me in many ways a mistake to consider social anthropology a *ding an sich*—an error to which many of its practitioners are prone.

The "anthropological model" used by most investigators whose work is discussed here is largely derived, either explicitly or implicitly, from Durkheim, Simmel, and Weber. These same figures have, of course, provided points of orientation for other social sciences—sociology, social psychology, and behavioral studies in political science. The first of the mechanisms that Eisenstadt mentions, "the interaction of the same people or groups in different situations," appears to be similar to what Simmel calls "multiple group-affiliations within a single group." Simmel gives, as a typical example of this, "the competition among persons who show their solidarity in other respects" (1955: 155). The discussion of conflict by Simmel and other sociologists (Coser 1956) has considerable relevance for anthropological developments (Gluckman 1954, 1956), just as his work on sociability, games, coquetry, and conversation has recently led Homans to speak of Simmel as "an ancestor of . . . small-group research" (1948: 597).

In view of the common theoretical sources and overlapping problems, it is not surprising that many convergences exist. Eisenstadt mentions work on "pri-

mary groups" and "role theory." On the other hand, there is a great paucity of effort directed to drawing together the results derived from work in these various fields. Parsons (1937, 1951; Parsons and Shils 1951) has tried to do so on the broadest level, and Homans (1951) on a more specific one; Nadel, in his posthumously published work, used material from sociology and social psychology as well as anthropology in his attempt to develop *The Theory of Social Structure* (1957). But then Nadel, unlike many anthropologists, was not content merely to cultivate his own garden; he wanted to see what was coming up in the field next door. In general, the attempt to develop a social anthropological approach—a process inevitably tied up with considerations of academic status—has often led to an emphasis on "difference" rather than similarity; and this in turn to a restriction of interests which has in some cases inhibited possible developments. Indeed, it has resulted in a limitation of techniques as well as of approaches; the social anthropologist is right to insist upon the advantages to be derived from the field-work methods he has customarily used, but to ignore other techniques is, as Nadel remarks, "obviously shortsighted" (1951: 7), and the sooner he gets over his "traditional distrusts" (Barnes 1959), the greater will be his contribution to the study of man.

Eisenstadt's review prompts this rather programmatic statement, because in some respects it points to the same conclusion. For example, to encourage the interchange and development of results and hypotheses, we need to take an intermediary step and put more emphasis on the sort of codification of results that Merton (1957) has so admirably performed for some areas of sociology. One form which such codification should take is the specification of widespread mechanisms of social life, but in such a way that their operations can be empirically validated. However, the analysis of such mechanisms, surely an essential preliminary to the development of any sort of "functional" theory (Merton 1957; Nagel 1957), should be of a more specific kind than that suggested by Eisenstadt. My main point bears on the future of social anthropology and is tangential to the article itself. Eisenstadt has attempted some drawing together of threads of the kind the field requires; on the other hand, one of the side-effects of treating the contribution of social anthropology as a one-way process may be to add to the growing isolationist tendencies and thus restrict the interchange so necessary to its further development. To paraphrase a recent remark of Evans-Pritchard's, most of our intellectual capital came from

earlier overseas investments; when those are exhausted, "we are in danger of falling into mere empiricism" (1960: 24).

By SOLON T. KIMBALL*

A critical evaluation of Eisenstadt's comprehensive and brilliant analysis of the contribution of social anthropology to the study of complex societies should probably be reserved for one or more of his British colleagues. Certainly, on the basis of the evidence which he adduces and of my own more limited knowledge of the development of social anthropology in England, he is justified in the conclusions to which he comes. It would be improper, dangerous, and erroneous to infer, however, that the development of social anthropology in the United States parallels that in England. It is a curious fact that the two traditions have diverged so widely in a quarter-century. Perhaps CURRENT ANTHROPOLOGY will some day publish, as a companion piece to Eisenstadt's paper, a definitive article in which this contrast may be drawn. In the meantime, I should like to make some observations on the direction in which some American social anthropologists have moved in handling problems connected with the analysis of complex societies.

The differential development of social anthropology in England and the United States is a fact of some significance, and has led to quite different consequences in the problems posed by the application of its methods to the study of complex societies. Quite early in the development of social anthropology among Americans, the concept of change became a central concern. Consequently, the theoretical tools, or "model," differed greatly from their counterpart in England as summarized by Eisenstadt. Emphasis upon norms and "regulative mechanisms" was replaced by attention to "systems" and their interdependencies. Inevitably, this led to concern with the "conditions" within which these systems operated, as sources of modifications which affected the characteristics and operation of a system. The techniques of field work began to alter with the shift from the search for functional generalities expressing the "direct interrelation of social behavior to group and institutional structure" to the specification of who does what with whom in time and space dimensions (Chapple 1940).

It seems to me that the difference in emphasis upon the concept of change represents the crucial point in explaining the divergence of British and American social anthropology. The insistence by Radcliffe-Brown (1952) upon synchronic analysis has apparently been retained as a central tenet by British social anthropologists. This position

was defended on the grounds that a science of social structure depended upon comparative analysis of institutional forms and mechanisms derived from the functional study of whole societies. The first studies by American social anthropologists adhered to the same position. The co-operative volume edited by Eggan (1937) was prepared as a tribute to Radcliffe-Brown by his students at the University of Chicago, and the separate contributions exhibit the mark of his intellectual fatherhood. In contrast, even those who had not been directly affected by the newer "functionalism" began to feel its effects. This is reflected in a volume sponsored by Linton (1940), which described acculturation among several American Indian tribes.

It should be remembered, however, that by 1930 Warner (Warner and Lunt 1941) had initiated the first major study utilizing a social-anthropological approach. The analysis presented in his first volume adheres closely to the cross-sectional synchronic approach. But when he (Warner and Low 1947) faced the task of analyzing the factory system of Yankee City, Warner either found it necessary or deliberately chose to present his findings in the context of historical change and of the relation to outside economic forces. He also utilized a single event as the point of analytic focus. It seems reasonable to conclude that, in this instance, the nature of the data required a modification of the synchronic and structurally deterministic position characteristic of the social anthropology of Radcliffe-Brown.

The rapid growth and acceptance in the 1930's of applied anthropology as a legitimate activity for anthropologists also undoubtedly had an incalculable effect. Anthropologists were offered opportunities to work in industry and government. The Bureau of Indian Affairs and the Department of Agriculture were particularly receptive to studies and counsel on problems of innovation in the period immediately preceding the war. In addition, a number of special studies, such as food habits and the education of Indian children, were undertaken. Basic to all such enterprises and studies was the concept of understanding social and cultural change.

Those who concerned themselves with these problems of contemporary society were considered to be either social or applied anthropologists, and in many instances the terms were used synonymously. In effect, response to influences and demands outside anthropology was shaping theory and technique.

Exemplification of this line of devel-

opment can be found among some whose studies were in strictly simple societies. Sharp's (1952) analysis of the consequences of the introduction of steel for stone axes among the Yir Yoront could be considered a classic example of the utilization of the concepts of system, interdependence, and change. In fact, the casebook in which it appeared (Spicer 1952) is entirely oriented in this direction. Oliver's (1955) analysis of status-giving feasts among the Siuai further exemplifies the applicability of analysis which depends upon recording who-does-what-with-whom-in-what-sequence-and-under-what-conditions as the necessary basic data for determining the interconnections and functions of institutional systems. Substantially the same technique of observing and analyzing was utilized in the Banks Wiring Room study of the Western Electric study (Roethlisberger and Dickson 1940) in the early thirties. A more general but equally fruitful utilization, based on examination of the broad canvas of societal characteristics and change, was achieved by Coon (1951) in his analysis of the Arabic cultures of North Africa and the Middle East. Arensberg has demonstrated the utility of the approach in several analyses of industrial and market microsystems, but has also successfully extended it to the characterization of American settlement patterns (Arensberg 1955). These references by no means exhaust the list of those who have found this a satisfactory approach for dealing with a wide variety of types of problems and levels of societal complexity.

Brief comment should also be offered about certain other specifics included by Eisenstadt. His examination of the value of the concept of "social field" leads him to grant that it does "tell us that we have to look for some general divisions of society and their interrelations . . ." but he then indicates a number of problem-areas where it does not help. The concept of social field has not been utilized by American social or applied anthropologists and, in my judgment, is incompatible with the concept of interdependent variables and hence unnecessary as a theoretical construct for purposes of analytical analysis. In contrast, the concept of "network," to which Eisenstadt responds favorably, has been used for nearly three decades and has been given greater specificity by linking it to actual events (Whyte 1943). In fact, the utilization of "event analysis" (Kimball and Pearsall 1955) as a technique for overcoming many of the difficulties inherent in the study of complex societies may

prove to be a valuable addition to method.

Social anthropology, as the discipline concerned with the structure and functioning of society, should supposedly be concerned with the "simultaneous operation of different types of regulative mechanisms within a given society." Eisenstadt feels that British social-anthropological studies have not been very successful in this area. I do not believe that comparable American studies have been very successful here either. Perhaps the real question is whether or not we should be too much concerned with this problem. Physicists have not yet been able to encompass the dynamics of the universe in one equation. Any such comparable achievement in the area of social science similarly appears unlikely in the immediate future. In the meantime, studies of complex societies can be concerned with more modest and manageable problems.

In conclusion, Eisenstadt is to be highly commended for a most intricate and careful analysis of the state of social anthropology among British anthropologists. If his article has merit in itself, it has double merit in providing the stimulus for a comparable analysis of social anthropology in the United States.

By EDMUND R. LEACH☆

Eisenstadt's essay seems to me out-of-balance. The crucial distinction on page 203 is between "total" or "tribal" societies on the one hand, and "complex" societies on the other. The criteria are not clear. No doubt there is a sense in which Londoners and Norwegians belong to "more complex" societies than do Nuer or Tallensi "tribesmen," but when Eisenstadt implies that Malay peasant society is "complex," while modern Uganda is not, the distinction is obscure. Social anthropologists have been paying close attention to African states for the past thirty years, but while Eisenstadt mentions Nadel on the Nupe as a study of "complexity," he treats Kuper on the Swazi as a study of "total society." He altogether ignores Schapera on the Tswana, Gluckman on the Barotse, or Fallers on the Soga—to name only three. Since Gluckman (1955) and Fallers (1956) went out of their way to point up the parallels between African and European institutions, the omission seems serious.

The references to Freedman's work might be thought to imply that social anthropologists have only recently turned their attention to China, but in fact the work of Fei Hsiao-tung and his colleagues (e.g. *Peasant Life in China*, *Earthbound China*, *China Enters the*

Machine Age, *The Golden Wing*, etc.) started around 1935, much of it being specifically concerned with the application of anthropological techniques to the study of industrialisation and urban migration.

Again, Eisenstadt's selection of writings on peasant India is very arbitrary. My own view would be that the most significant recent contributions towards the understanding of this kind of complexity have come from Gough (see bibliography in Gough 1959), Dumont (1957), Dumont and Pocock, and Barth (1960). Eisenstadt mentions none of these.

I also complain at the deceptive references to social anthropology in France. Lévi-Strauss is mentioned at the beginning and again at the end in contexts which suggest that his work is of the same general kind as that of the British authors discussed more fully in the body of the essay. This is untrue in a quite fundamental way.

Two further points seem to me misleading. The quotation from Barnes (1959) on page 205 is quite inapplicable to the set of studies mentioned in the previous paragraph. Thus, Freedman (1958) is not based on firsthand field work at all, while Bott (1957) is of especial interest precisely because the methodology of research ran directly contrary to the scheme of procedures here indicated.

Finally, I find it very odd that Eisenstadt should consider that social anthropologists have failed to make a critical appraisal of the concept of "role." Nadel (1957) is almost wholly preoccupied with the applicability of this concept to anthropological studies; and I should have supposed that most social anthropologists are now very fully alert both to the value and to the limitations of this concept as a tool of analysis.

By DAVID G. MANDELBAUM☆

Eisenstadt's noteworthy survey suggests to me certain implications which deserve mention. It shows how markedly the range of studies in social anthropology has broadened to include more work than was formerly done on complex societies. These recent studies are impressive in number and quality. Eisenstadt legitimately confines this paper to the work of British social anthropologists, although he does mention some similar and converging trends in the work of anthropologists of other affiliation and nationality. Since the concepts of "social network" and "social field," as discussed in this paper, apply also to anthropologists, it would be useful in a further survey of this kind to include the broader and more complex setting.

Implicit in this paper is a contrast between these social-anthropological studies and the studies of complex societies which have long been made by historians, political scientists, sociologists, and others. While the contrast is implicit, the answer to the question which rises from the contrast is quite explicitly given. Does social anthropology have a distinctive and useful contribution to make toward the understanding of complex (i.e., non-tribal) societies? Its contribution is both distinctive and very useful, Eisenstadt concludes. The method of field work and the main model of analytic procedure are used to produce studies which combine description and analysis as no other studies of social behavior do. Although this method and model were developed in the study of tribal peoples, they have been usefully applied to the study of civilizational societies, being especially fruitful in showing the continuing importance in those societies of domestic, kin, and local groups, of basic images, and of institutional interrelations. Moreover, through their research in the civilizational sphere, social anthropologists have become more cognizant of those social forces and bonds which are not encompassed within particular groups—and may not even be integral elements of any specific group—but which can powerfully affect behavior.

In showing how similar social processes operate in both tribal and complex societies, these studies provide corrective evidence for the commonly used, and over-rigid, dichotomies of sociological classification. Anthropologists can take particular note of Eisenstadt's statement in Section VIII that these studies have contributed to the reassessment of the sociological image of modern societies. The method and the model of social anthropology are thus useful for the study of all societies—for all types of contemporary societies, for analyzing evidence of past societies, perhaps even (though Eisenstadt does not say this) for extrapolating general processes to indicate the possibilities for future trends. But, useful as this approach is, it is not yet fully enough adapted to the study of complex societies.

Social anthropology, we may infer from this thoughtful survey, offers the advantages and the defects which are inherent in its *stage* of development as a discipline. That stage has been achieved by focusing on functioning groups and clear groupings, by tracing the relations among groups and institutions, by noting the contribution of individuals and institutions to the maintenance of the groups. This was done in the context of a total society, taken as relatively homogeneous and

relatively isolated. Only passing notice is usually taken, at this stage, of the impact of forces external to the group being studied, of internal variations, of alternative choices, of situational differences in structural alignments.

This procedure serves well in formulating the structural anatomy of a social entity; crucial articulations of the component parts are shown, and the contributions of institutions and of individuals to structural maintenance are demonstrated. The resultant analysis is given living semblance by appropriate examples from real behavior. It can be argued that this simplification was not only a first stage in the development of social anthropology, but also a necessary first step in the analysis of any social entity.

Beyond the first stage or the first step, the need for further developments of scope and of concept become apparent. Second-stage models are in order, not only because of the nature of complex societies, but also because of the rejection (often violent, sometimes premature) of analyses based on the simplified social model. In scope, the local community, the unit most frequently taken for observation, is both too narrow and too gross. It is too narrow because the larger social forces which affect its members cannot be adequately understood from the study of any one community. It is too gross because concentration on social morphology tends to slight the social choices and alternative courses which are powerful factors in individual behavior and in social change. Not that these matters have been ignored in the first-stage studies; they have not been systematically incorporated into the analysis. These problems of scope are succinctly illustrated in the paragraph of Eisenstadt's section V which discusses Barnes' study of a Norwegian parish.

The social anthropologist can augment his close field-observations by drawing on relevant studies in other disciplines which do treat of the over-all society. His work, in turn, has augmented and stimulated studies in the other disciplines. Even more important, as Eisenstadt shows, is the development of concepts which will help link individual behavior, group structure, and institutional activities. A main concept to be developed is that of "role." Originally propounded by Linton, theoretically sharpened by Nadel, further developed by various sociologists, it has not yet been used to full effect in anthropological analysis. We can agree with Eisenstadt that it is a key concept at this juncture; it may not be too much to say that as the concept of atom is to molecule, and as gene is to chromosome, so is role to structure.

Effective use of the concept of role

may well facilitate more powerful use of the newer concepts mentioned by Eisenstadt: social network, social field, and social organization. All should be used to illuminate the choices, contradictions, and conflicts which are at the root of social and cultural change. And concepts which deal with social changes over time will add notably to the scientific value of the structural analyses which have already proved their usefulness in the study of complex societies.

By DAVID M. SCHNEIDER*

Eisenstadt's appraisal of the contributions of the English social anthropologists whom he lists, and his masterly summary of the kinds of work they have undertaken, are very welcome indeed; for such an appraisal and a summary have been very much needed of late. That he is a trifle more generous in giving credit than an obsessive devotion to hard facts might warrant goes without saying. I do not find it possible either to add to or to detract from his treatment of the subject.

I would raise only one point: Eisenstadt begs the question of dealing directly with "complex societies" and what they are. He does not define this category; nor does he do more than distinguish it from "primitive," without defining that category either. I am puzzled by his classing the small Norwegian hamlet studied by John Barnes with the Nupe, and the Nupe with southeast China, London, and Malabar. Are the Tswana "complex" or just numerous? Is the Murngin social system "complex" or just difficult to understand? What is the distinction between "modern" and "complex" which is introduced at one point? Does this mean that the contemporary Africans whom Gluckman studied were "complex" but not "modern," while the Pentredwaithians, the primitive Welsh group so devoted to football, are both "complex" and "modern"?

If this were a minor problem one would hesitate to quibble. But in the present context it is a problem on two counts: First, it is a major problem which social theory has clearly failed to cope with, yet one of the most pressing of the problems before social theory at this time. Second, it is not easy to appraise properly the contributions of English social anthropology to an undefined entity.

Eisenstadt's treatment of this problem of "complexity" seems characteristic of the treatment generally accorded it today, which consists mainly in simply referring to "complexity" and then ignoring the matter. One suspects that a professional bias might be involved.

The profession of sociology has been reared in a climate of opinion which still implicitly accepts as a fact the idea that "modern" "mass" "industrial" "complex" society is a thing apart, an entity in itself, a radical departure from all other forms to be found on earth. This was undeniably the assumption of Weber, Durkheim, Freud, Spencer, and, indeed, of many if not all 19th-century anthropologists. Although 20th-century sociologists have learned to hold their tongues when they are about to utter such impieties as "primitive people" or "primitive state of society," they are on the whole the same persons who vehemently assert that one just cannot compare kinship in a "modern" "industrial" "mass" "urbanized" "complex" society with kinship among the Eskimo, or the idea of Fate among the Greeks with the idea of Fate among the Tallensi.

Evidence of this bias in Eisenstadt's paper is seen in his surprise that

regulative mechanisms, *belonging as it were to one type of society*, [my italics, double emphasis on 'one type of society'] operate in some parts of other types of societies and constitute basic components of social organization.

Starting from precisely the opposite bias myself, I find this not surprising at all. I should have seriously doubted the professional competence of those who failed to find evidence of such regulative mechanisms.

Again, Eisenstadt seems to have learned from anthropologists that

modern society is not a 'mechanized' one, in which atomized individuals live in separation, ruled only by impersonal forces. Rather, various closely interwoven personal and group relations, on the one hand, and relations permeated with symbolic and primordial meanings, on the other hand, constitute basic components of even the most differentiated type of society.

How, in 1961, is it possible for anyone to believe that "modern" or any other society could conceivably be "mechanized," and therefore be surprised that "modern" society, like all other societies of which we have any knowledge, entails closely interwoven personal and group relations, symbolized and meaningful? One might suspect a residuum of Herbert Spencer lurking about and fouling the sociological atmosphere.

The fundamental problem is that of dealing with the range of different kinds of societies within the framework of a theory of society. It is precisely Weber's technique of putting one case in a class by itself, and lumping all other cases into another class, which has tended to yield, in the hands of others, the patently erroneous conclusion that the one

class must be of a drastically different order from all others in *all* relevant respects. For the problems Weber dealt with, his separation of the one class from all others may well have been legitimate. But it has had the unintended and highly undesirable consequence of fostering the illegitimate conclusion that in all *other* respects, too, the one class must also be radically different from all other societies. Eisenstadt has expanded the one class slightly, but without redefining it, and, I believe, has fallen into precisely this error. Otherwise I find it difficult to explain his designating as "contributions" of English social anthropology what seem to me to be commonplace and reasonable expectations about the structure of *any* society, regardless of its complexity, modernity, degree of industrialization, urbanization, or the magnitude of its mass.

By LAILA SHUKRY EL HAMAMSY☆

Eisenstadt's review of the contribution of social anthropology to the study of complex societies comes at a well-chosen moment. There is no doubt that the time has come for social anthropology to scrutinize itself and question its earlier specialization as a discipline concerned with so-called "primitive," "simple," "tribal" societies. World-wide developments clearly indicate that anthropologists would need to look far and wide to find such societies today—societies which are not themselves developing greater complexity, or that are untouched by forces originating in more complex societies. The examination of social anthropological approaches to the study of complex societies, therefore, should be of concern not only to a special group of anthropologists interested in complex societies but to the whole field of anthropology. The designation of a tribal group or subgroup for study as if it were an isolated social unit is of course possible, but would, I believe, offend one of the best anthropological traditions and one of anthropology's most important contributions to the other fields of social science: namely, the insistence upon placing any social unit in its total cultural context. Inevitably, the total cultural setting of most societies in existence today includes the impact of complex social, economic, and political forces.

Eisenstadt has very aptly analyzed and evaluated the special theoretical model—and, in an indirect way, the method of research—that social anthropology has contributed to the study of complex societies. He has also clearly pointed out the shortcomings of social anthropology in trying, through the application of its theoretical model and

related concepts, to analyze and relate the multiplicity of forces at work in these societies.

I have little to add to Eisenstadt's analysis, but I should like to present some practical suggestions that may help social anthropologists solve some of the problems Eisenstadt raises. First of all, I suggest the fruitfulness of collaboration between social anthropology and other fields of social science in an interdisciplinary approach to the study of those problem areas which Eisenstadt has implied would require more refined tools of analysis and a more sophisticated theoretical framework as well as, perhaps, better techniques of research. Other social sciences have been developing their own methods and theoretical models for the study of complex societies, though they have usually limited their studies to the types of phenomena which fall within their specialized spheres of interest. Social anthropology could make use of the various insights gleaned by other social sciences and, in return, through the use of its traditionally functionalist and holistic approach, help integrate these into a body of theory and a set of analytical tools that would explain the workings of a complex society.

In pointing out the areas which social anthropology has failed to analyze adequately in its study of complex societies, Eisenstadt is at the same time pointing out important areas for future research. One area of research which I submit is a fruitful one for those interested in an understanding of the mechanisms regulating "the interrelations of social behavior to group and institutional structure" in complex societies, is the study of power-groups or "government" in the broadest sense. This would be particularly important in the so-called under-developed areas of the world, where greater centralization of power accompanying a developing nationalism is forcing the integration of tribal and folk groups into state structures. This centralization of power has resulted in the emergence of power-wielding institutions and groups which encroach upon, and may dominate, all other institutions of the society. These centers of power regulate behavior and induce changes in the structure and functions of existing institutions. They also impose conformity in culturally heterogeneous groups by setting up and enforcing norms of behavior, either through coercion and legal sanctions or through persuasion (through propaganda via mass media of communication, the creation of ritual symbols, and the like). Such power-groups consciously plan changes in behavior and institutional organization, and impose standards of conduct which may conflict with pre-

existing cultural values. A study of the internal organization of power-groups, the value systems of the participants, and the particular influences that dominate their behavior, as well as the methods they use to influence the rest of society, are very important for an understanding of some of the forces at work in complex societies. Some anthropologists have studied the effect of the central power structure on subgroups. Has the time not come to turn our attention, in collaboration with other social scientists, to studies of the centers of power themselves?

By INA E. SLAMET-VELSINK☆

The problems raised by Eisenstadt are no doubt of crucial importance to present-day social anthropologists. I was directly confronted with them a couple of years ago, when I started doing research in a Central Javanese village of the Klaten region; and ever since, I have been trying to elucidate the principal questions involved in the analysis of local Indonesian cultures from the viewpoint of modern social science theories and methods.

Although I think Eisenstadt's article is highly stimulating, I should like to formulate some points in a different way, so as to throw, I hope, another light upon them.

First and foremost I wish to acknowledge, with Eisenstadt, the important contributions made to modern anthropology by the specific branch of social anthropology he discusses: especially the patient, conscientious, and detailed field work done by its major representatives, and the strictness and refinement of the methods used. These achievements have obviously advanced the development of the social sciences in general to a very considerable extent, at the same time correcting the tendencies toward broad generalizations without factual justification, as well as the neglect of the systematic study of concrete and specific interrelations, which were inherent in most of the older anthropological literature.

Nevertheless, the social anthropological approach outlined by Eisenstadt has serious limitations, which have always been present but are becoming more evident and troublesome in the face of complex and/or rapidly changing societies. In my opinion, these shortcomings are not inherent in social anthropology as a discipline, but are due to an exclusively microsocial orientation as well as to an almost pathological fear of evolutionism. These assumptions are easily stated, of course and say nothing original; but their consequences should be fully realized, not only because the study of complex societies has been severely handicapped by this lack of

perspective, but because it has frequently impaired even studies of relatively small social entities. An intensive study of the uniformity, as against the variety, of historical development, and of the dialectics of social change (including processes of diffusion), might well prove to be a prerequisite for the study of complex societies in particular, and a necessary requirement for understanding the exact nature of, for instance, the choice that individuals are offered among different and contradictory institutional forces and groups; or a necessary requirement for grasping the full meaning of political and economic conflicts between Europeans and Americans on the one hand, and Africans, Asians, and Latin Americans on the other hand; and explaining why these conflicts could never contribute to the continuous integration of an ongoing society in any way.

This does not mean that social anthropologists should turn away from the painstaking study of the real behavior of human groups and individuals in concrete historical situations, and of the norms that govern or fail to govern such behavior. On the contrary, these studies, if combined with broader theoretical views and interests, may be able to bring about, among other things, the badly needed refinement and sharpening of evolutionary theory and a deepening of insight into what really happens, on the physical as well as on the spiritual plane, at all levels and in every part of society in the course of minor or major social changes or even revolutions. In other words, these studies, if brought to the point, might singularly advance our comprehension of the dialectics of social change.

By some illustrative material, closely related to my own field of experience, I shall try to make clear the practical significance of the views set forth above.

Leach (1954), in his study of the political systems of Highland Burma, struggled with the problems of the interrelations and mutual interdependence of three types of social and political organization: the equalitarian, segmentary type with no political units larger than village-republics (*gumlao*-type); the authoritarian, hierarchical type, characterized by aggregates of villages under a common chief, by a class system of an almost caste-like rigidity but still contained within the framework of a segmentary tribal society, by endless feuding and general instability (*gumsa*-type); and the third type of feudal or feudal-like states with centralized authority, dominant in the plains and valleys where agriculture is based on wet-rice culture in contrast to the shifting cultivation still extensively practised in the hills. Leach, on

the one hand, does not neglect to take the ecological, economic, and general historical conditions into account in analysing the processes of change going on in his composite society. On the other hand, however, he repeatedly presents the situation he depicts as one offering individuals, as well as groups, a choice between different political institutions; and the dynamics of Kachin political organization as a continuous oscillation between the *gumsa* and the *gumlao* type of government.

I mention the work of Leach for two reasons: first, it is one of the few studies by a social anthropologist that attacks the problem of complex societies from a rather broad angle; second, the subject matter of his book is very familiar to me, because in several aspects it is strikingly similar to Indonesian conditions, so as to make discussion of methods and interpretations more fruitful.

I shall limit myself to only one of the several interesting questions raised by Leach, i.e., the oscillation between *gumlao* and *gumsa* type of organization. I admit that from the individual point of view the choice between the two traditions concerned may present itself as a simple alternative; but historically we are confronted here with a case of irreversible social evolution, as amply born out by the Indonesian facts. The *gumsa* type of organization may be called characteristic of all the Dong'son-megalithic cultures that still exist, existed until recently, or existed in prehistoric times in present-day Indonesian territory. In several instances (Batak, Toradja), former equalitarian traditions are still clearly preserved in other sections of the same ethnic groups or in the folklore (Kruyt and Adriani 1912; Kruyt 1938; Vergouwen 1933), whereas the transition to a full-blown caste system and a political organization of petty feudal kingdoms under Hindu influence is still traceable in the Balinese case (Korn 1932, 1933). Cultures that are directly comparable to the *gumsa* type described by Leach are still partly extant in Nias (Schröder 1917). Flores (Arndt 1929, 1931, 1932) and Eastern Sumba (Nooteboom 1940), but there is strong evidence that South Sumatra and at least the greater part of Java were politically organized in a similar way before the Hindu period and, in some regions, even much later (Thomassen à Theussink van der Hoop 1932, 1938; Heekeren 1958).

The most interesting point in this special case, however, is not the general evolutionary trend from a segmentary tribal organization to a patriarchal one, becoming increasingly feudal in the course of time (cf. Lévi-Strauss 1949:

301); nor even the internal and external factors that make this trend operative at different times, in different places, and in different forms; but rather how Leach came to consider the transition from *gumlao* to *gumsa* type of organization and vice versa as an oscillation. The *gumlao* revolts he mentions were no doubt real, as was—and is—the constant clash between the two contrasting traditions; but the process involved might be far more complex and consequential than Leach seems to believe. The *gumlao* revolts are clearly peasant revolts against nascent feudalism, or feudal-combined-with-colonial oppression; and it is, indeed, not uncommon for the goals of such revolts to be conceived and formulated in terms of an idealized past; but this does not mean that the process of historical development can ever be reversed or that such a reversal is even really desired by those who reject the present. And especially in the case of the *gumlao* revolt predicted by Leach for the near future, one should like to know much more about the process of reinterpretation of the old values, which has certainly transformed the old *gumlao* ideals in recent times, and about the present-day situation and the composition of the groups supporting these ideals. Why is it so hard for these *gumlao* patterns of organization to die? In what sections of the society, and in what sort of activities, were they so entrenched as to be able to resist all feudal and colonial pressure? How far was their content modified, and what are their future potentialities, if any?

As far as my experience goes, these and other questions cannot be answered by scrupulous field work alone, or by knowledge of general evolutionary theory; their solution can be brought nearer only by microsocial investigation combined with a thorough study of the special characteristics and the specific evolution of the cultures in the area concerned. Without an understanding of the special nature of slavery among the Kachins, most of the Indonesian peoples, and many other Oriental peoples, as compared to the same institution in classical Europe; without an insight into the particular variety of feudalism predominant in regions economically dependent on large-scale irrigation; and without an objective evaluation of colonial and semicolonial conditions and their impact on the several kinds of indigenous societies, the persistence of archaic equalitarian traditions of organization and their changing role in rural life may become quite incomprehensible.

On the other hand, it seems that com-

plete familiarity with the sociological and historical background of the culture concerned, and with the general structural and evolutionary problems involved, cannot prevent experts from misinterpreting facts relating to the "little tradition" (in the Redfieldian sense) in a rather frightening way. I am thinking, for instance, of Wittfogel's (1957) evaluation of Chinese village traditions. General judgments, such as "total submission" and "total terror," must seem completely inadequate terms for these traditions to any social anthropologist who has ever made a real effort to understand the complex system of values and attitudes of a poor peasant population of an Asian village in a "hydraulic key-area"—that is, one who did not content himself with simply accepting their avowed attitudes towards their overlords as a general norm.

In the Javanese case, a comparison between the conclusions of Van der Kroef (1951) and Clifford Geertz (1956) may bring out at once the need to supplement studies of written sources and general experience with a given culture by careful and patient anthropological research. In a Javanese village, archaic equalitarian patterns of leadership and mutual aid may be nothing more than glowing embers, hidden in unspectacular and seemingly minor institutions, after centuries of repression by feudalism and colonialism alike. But they are not dead; and although I do not share all of Geertz's views, I fully agree with him about the importance and tenacity of these patterns. They have, indeed, not only been repressed, but also indirectly strengthened by feudal and colonial exploitation; and they have played and are still playing the crucial role of preventing the poorer peasants from starving, and even—as I was able to observe myself in some cases—from losing their sense of self-esteem through their inability to meet the customary ritual obligations or through coarse treatment—according to local standards—by kin or neighbours.

In this context, no more than the crudest outlines of the problem can be sketched. But it may be remembered that the issue is of prime importance from the theoretical as well as from the practical point of view.

I assume that, in China, millenia-old traditions were consciously revived, reinterpreted, and reshaped by the communists in order to smash the feudal system and to undermine the deep-rooted patriarchal institutions. And although in Indonesia the goals are partly, and the circumstances markedly, different, the same kind of *gumlaos* institutions are officially called upon to help carry out the land reform and to support the movement for national recon-

struction. Goals and means are still interpreted rather differently by different sections of the population, and the outcome may still be a matter of hot debate, but the importance of the phenomena is undeniable.

So I think that social anthropology still has an important role to play in studying complex societies, but the task is not an easy one. In the majority of rural cultures in underdeveloped countries at the present day, the complexity of various planes of organization, superposed and intertwined in a network of intricate interrelations and contradictions without clear-cut boundaries; the clash of old values and symbols with new social conditions and the manifold reactions ensuing from these antagonisms; and the sometimes radical shifts in group-alignment if compared to the traditional status system: these phenomena are all but dazzling.

Nevertheless, and if only because of the vital issues involved, this situation should not induce us to give up any of the *raison d'être* of our discipline as a distinct science, which are: the study of the growth and development of culture and cultures in time; the study of societies and their cultures as structural and organic wholes, and their comparison; and the claim to comprehend objectively and to translate accurately the individual and collective life-experience of human beings acting, thinking, and feeling in cultural settings other than our own. Never before, indeed, has there been such an urgent need to promote a closer integration and a reciprocal fertilization between the different fields of anthropology, as well as between anthropology and other related disciplines.

By P. M. WORSLEY*

Eisenstadt's article indicates that, whatever the achievements of social anthropology, past and present, serious reconsideration of its basic postulates is long overdue if it is to have any future. Even in his first great classic, in 1922, Malinowski ominously began by remarking (page xv):

Ethnology is in the sadly ludicrous, not to say tragic, position, that at the very moment when it begins to put its workshop in order . . . the material of its study melts away with hopeless rapidity.

Nor is the attempt of CURRENT ANTHROPOLOGY to stimulate the subject by pushing it back 100 years into an unholy alliance with archaeology and physical anthropology likely to satisfy the social or cultural anthropologist who is increasingly worried by doubts about the validity of orthodox assumptions in, and about, his science.

This crisis is the product of two inter-related phenomena: a change in subject matter and a change in theoretical postulates. Whether we consider that the classic anthropologist devoted himself primarily to "primitive" or to "colonial" social situations, or both, he now must consider mining towns and political parties as well as kinship systems. Now, "barbarology"—to accept the jibe—was and is a perfectly valid science; but the assumptions built up in the study of relatively stationary, closed societies prove inadequate for the study of the contemporary world. Indeed, it was the study of industrializing, conflict-laden southern Africa which stimulated the first trenchant critique of the holistic extremes of modern structural-functionalism from within social anthropology itself, by Gluckman (1940, 1942, 1949).¹ As a result, Gluckman was led to push his analysis further, towards a general analysis of *conflict* as a fundamental process in social life.

Face to face with the even greater differentiation of modern industrial society, many anthropologists initially restricted themselves to the study of circumscribable *milieux*, to use Mills' (1959:8) term: the small group, the enclave, the village, the Negro in the city. This patently inadequate definition-of-the-subject-by-implication set up a further reaction: anthropology was, now, chiefly a *technique*—"micro-sociology." Both conceptions are fused in Barnes' phrase "politics round the village pump."

In the end, the "technicalists" are perhaps nearer the truth. The subject matter as traditionally known is "melting away with hopeless rapidity." But the conclusions that ought to be drawn are not the restriction of the subject to the study of limited *milieux* within an unexamined general social framework, nor the reduction of the subject to the status of a mere technique. Instead, we should be expanding the subject towards its fusion with sociology and related social sciences, even if these themselves are also in need of parallel reorientations. It is quite true, of course, that anthropologists have long been researching in restricted sectors of the "modern" society. The more enterprising anthropological theorists (e.g. Nadel, Bidney, etc.), too, have increasingly turned outwards, away from the theoretical inbreeding of the 1930's. Sociologists, for their part, are beginning to contemplate the world outside western Europe and North America, and increasingly utilize "anthropologi-

¹ I have attempted to spell this out in greater detail in "The Analysis of Rebellions and Revolutions in Modern British Social Anthropology" (*Proceedings of the 4th World Congress of Sociology*).

cal" techniques. Unfortunately, the universities have by now absorbed the anthropology of the 1930's, and regard the setting up of new and separate departments of anthropology as a *sine qua non* in demonstrating their maturity in the social sciences.

Eisenstadt is correct in referring to the "distinct" contribution which anthropology has brought to the study of complex societies, though his list of studies is perhaps formidable enough to distract from his major conclusions that these successes are very limited in province, and that the key concepts elaborated "do not help greatly . . ." So that while the "technicalists" have been realistic about the change in subject matter, they have not said as clearly as he has that what anthropology has had to offer *qua* technique is very limited. Whatever mechanisms and concepts have been developed (e.g. the three Eisenstadt mentions), and whatever the value of new emphases upon process and conflict, etc., these are isolated from any integrated general theory of roles-in-change. Oddly enough, Eisenstadt notes this absence of any *systematic role-theory* as the major deficiency in current anthropological theory, yet neglects to mention Nadel's (1957) pioneering work in precisely this field.

Orthodox structural-functionalists generally assumed a static-dynamics model which postulated a theory of structure and a separate theory of change.² It was also relativistic: it informed us, on one page, that ruling groups frequently oppose social change; and on another, that they often welcome it. Nadel, virtually alone amongst the anthropological theorists, embarked on the needed analysis of role-change, role incompatibility, the rejection and redefinition of roles, etc. Unfortunately, in avoiding relativism, he

pushed further and further, searching for propositions of higher and higher levels of generality, towards the very formal sociology he so capably criticized.³

A search for absolute propositions of this kind probably shows its weakest aspect when called upon to help us analyze processes of change that are highly specific to particular types of social formation and to particular phases of social development. This is most evident in rapidly changing societies which are closely affected by the growing interdependence of the world: e.g. the growth of political parties in the "emergent" countries; the lineaments of the "Organization Man" in neo-corporate society; the interest-group struggles in post-Stalin Russia. All this, of course, implies the setting of the specific problem or situation in the context of its place in human history.⁴ Neither relativism nor formal sociology has any such general framework to offer. By avoiding the delineation of these generic processes of change, we end up with either the relativist's culture-bound, over-specific (and often contradictory) propositions, or increasingly general statements empty of specificity ("groups have leaders"). Paradoxically, the point which structural-functionalism laboured so hard—the analysis of the forces making for the integration of society—is perhaps the anthropologist's greatest weakness when he turns to the complex society. He is nurtured on a theoretical model which subordinated the analysis of power to the study of reciprocity;⁵ which muted the facts of interest, force and conflict; and which highlighted complementariness and integrative value-systems. So, in social situations, where the key decisions are taken *outside* the milieu; where "commanding heights" exist; where the decisions may be taken by remote elites rather than by men in face-to-face relationships; where the decisions may be imposed and one-sided: in all these situations, the primary-group orienta-

tion is no longer adequate. Equally, where the value-systems are not unitary, and the "primordial symbols" themselves are objects of manipulation and redefinition, orthodox structural-functionalism cannot help us. Current studies of "traditional" societies demonstrate that the examination of power, influence, and conflict is central to any comprehension of social processes, even for this type of society.⁶

What has been achieved by anthropologists working in the "complex" field has largely been the product of sharp empirical observation coupled with a largely unexamined *ad hoc* modification of received concepts. The result is an uneasy compromise, arising from underlying conceptual unclarity. I don't think this state of affairs is any cause for self-congratulation.

² E.g., Hart and Pilling (1960) on the Tiwi; Turner (1957) on the Lunda.

Reply

By S. N. EISENSTADT

Since a full and satisfactory reply to the wide range of comments would necessitate another article, I shall limit myself to certain selected points.

(1) It was not my intention to write any kind of history, survey, or analysis of social anthropology in general or of British social anthropology in particular. My main concern was to analyze the application of (if I may paraphrase Fortes) an analytical construct or model to the study of a certain type of social phenomenon, i.e., "complex societies." This model has been widely used by many—although certainly not all—British social anthropologists, and it was most convenient to draw principally upon their work. The references cited were mainly used to illustrate different aspects of the model and its application, and were never intended to be exhaustive. However, I agree that I should have mentioned Fallers' and Southall's studies, because they illustrate certain interesting applications of this model; unfortunately, the publication on caste recently edited by Leach was not yet before me, and therefore I could not comment on it. Mention of Warner's *Black Civilization* would certainly have been in place; but I did not mention the Yankee City studies because to my mind they constitute an application of some of the concepts and research tools used in social anthropology, rather than of the analytical model in its entirety. Similarly, I did not refer to Nadel's con-

² Where this dichotomy leads one is indicated by this sentence in an otherwise excellent field-monograph (Willmott 1960), summing up a chapter on "Community Structure":

"In this chapter the analysis has been almost entirely structural. Very little attempt has been made to discern changes. . . ." (p. 128).

Yet the chapter includes the following passages:

"They tend to lose their regional loyalties . . ."; "the nationalist movement which grew up. . . ." [p.99]; "this practice is often omitted today . . ."; "the postwar generation are taking a very active role in selecting their own spouses" [p.100]; "educational differences are diminishing . . ." [p.106]; "a new distinction is emerging . . ." [p.115]; "motorcycles and leather jackets . . . have become the outward symbol of [the moderately wealthy youth]" [p.125]; etc. (emphasis added).

³ E.g. in his criticisms of Chapple and Coon, Homans, and Simmel *et al.*, in *The Theory of Social Structure*.

⁴ Cf. Mills (1959:128): "Classic social science . . . neither 'builds up' from microscopic study nor 'deduces down' from conceptual elaboration . . . [It takes up] substantive problems on the historical level of reality" (emphasis added). Some anthropologists are beginning to spell this out in their field studies. For example, Burridge (1960:112) notes: "Behind Tangu as we find them today lie many years of continuous development and change. . . . Indeed, to argue from a presumption of stability in Tangu during the last century and a half would be most ill-advised."

⁵ Cf. Gellner 1958.

tribution to role-analysis theory because it was rather immaterial from the point of view of my analysis, which mainly concerned the extent to which anthropologists have used this concept in their studies of complex societies. It is, of course, obvious that other types of anthropological studies of complex societies exist—mostly, though not exclusively, undertaken by American anthropologists—and therefore I entirely agree with Kimball that an attempt to evaluate these would be worthwhile.

(2) My main concern was to point out, from the perspective of a sociologist looking at social anthropological studies, the main contributions and limitations of studies of "complex" societies that used this model (or perhaps, as Fortes suggests, two interconnected models). I wanted to show that the use of this model had made very specific contributions to the understanding of complex societies, but that these contributions were limited by the very strength of the model; and that in order to overcome these limitations it was necessary to contrapose this model with other models that had been developed within other social sciences.

Beyond the analysis of some of the concepts developed in these studies, I did not intend to specify the exact ways in which such limitations can be overcome. Most of the suggestions made by El Hamamsy, Worsley, Barnes and Epstein, and Slamet-Velsink seem to be very pertinent, although of course they do not necessarily exhaust all the possibilities. I certainly agree with Goody about the necessity to look for various specific mechanisms, and with Mandelbaum about the importance of developing "secondary models."

While Schneider's views about the unity of mankind and the basic similarities of different human societies and of the regulative mechanisms operating within them are certainly correct, the specific confrontation of different models which emphasize such various mechanisms has been rare and could be very fruitful. Fortes' suggestion about the Christmas festival in England, for instance, is very stimulating from this point of view.

(3) The preceding section is closely connected with the important problem, raised by Schneider and Leach, of the difficulty of defining "complex" societies, as distinct from simple (or tribal) societies. Certainly both terms, to some extent, constitute residual categories. But it is possible that an explicit analysis of the major characteristics attributed to each of these types, as well as the analytical models implied in them, may greatly enhance our understanding of the extent to which it is useful or meaningful to employ these categories.

(4) It is of course, well known—as I should have noted—that this model was largely derived from Durkheim, although in its application it goes beyond him. However, I do not agree with Goody that it is also derived from Simmel. It is true that one of its implicit mechanisms was extensively analyzed by Simmel, but the distinct characteristic of this social-anthropological model is the combination of all the various mechanisms.

(5) Barnes and Epstein criticized my distinction between description and analysis. However, my very point was that most of the studies I cited have combined description and analysis, and that only by drawing, in so far as this is possible, a clear distinction between the two can we arrive at definite hypotheses about "laws" of social behavior that can also help us understand any specific situation.

(6) I agree with Beattie's point that a total society can never constitute a legitimate datum of analysis. However, it seems to me that many of those who have used the model analyzed here have very often, through the strong emphasis on social integration as an explanation of phenomena, unwittingly used total society as such a datum. Consequently, it would certainly be worthwhile to break up this concept into clearly distinguishable variables.

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The Study of Social and Religious Systems in North American Archaeology

by William H. Sears

UNTIL VERY RECENTLY, the problems in North American archaeology that needed immediate attention were the definition of cultural units through classification of artifacts, and the chronological ordering of the units and artifact classes. Most of the time, thought, and attention of persons working in this field is still given to description, basic artifact typology, definition of cultural units of varying dimensions, and arrangement of these units in time and space.

I need not present exhaustive analysis here to document the point; Taylor did this most efficiently in 1948. Relatively recent examples include the reports on the lower Mississippi Valley by James A. Ford and his associates in the American Museum of Natural History series; the report on the Central Mississippi Valley archaeological survey in the Peabody Museum Papers; and the Yale publications on Florida archaeology. The reports on River Basin salvage archaeology in Bureau of American Ethnology publications and in *American Antiquity* are other examples, covering a wide range of North America. Descriptive material dominates the state and regional journals and is common in our national journal, *American Antiquity*.

Such work is basic to anything else in archaeology, and in no specific instances are the citations above intended as criticism. However, except for formal and seldom-used descriptions of houses, burials, and site

plans, the determination of time and space positions and sequence building through manipulation of increasingly refined techniques of artifact style analysis has become an end in itself. Through a sort of feed-back reaction that causes archaeologists already engaged in this work to see only more problems of the same sort, the known content of many archaeological "cultures" of the United States is limited entirely to ceramic types, or includes only a few other types of artifacts. The depiction of "cultures" in such terms facilitates their identification and manipulation in time and space, but impairs their usefulness in the study of cultural and social form and process.

During the last ten years, a number of papers and monographs have decried this persistent emphasis on description and on the chronology of artifact types or pot-defined cultures, and have suggested that archaeologists might engage in other activities for their own benefit and that of other anthropologists. Perhaps we are seeing the first stages of a process similar to that Clark (1953: 352) describes for England:

Prehistorians of the older school paid attention chiefly to the formal, typological characteristics of such material, concentrating on features of value for classification. The modern school is more realistic; it is concerned less with the ideal categories of modern scholars and more with what really happened in prehistoric times.

The upper or more complex level of these new activities has been called "processual interpretation" by Willey and Phillips (1958: 5-6), who use the term for

the study of the nature of what is vaguely referred to as the culture-historical process. Practically speaking, it implies an attempt to discover regularities in the relationships given by the methods of culture-historical integration.

The clearest and most precise delineation of the levels of archaeological interpretation is presented by MacWhite (1956), who breaks down in some detail the gross divisions of "chronicle" and "historiography" outlined by Taylor (1948), and shows the relations of the operational levels subsumed under the Willey and Phillips (1958: 4) triad of (1) observation, or field work; (2) description or culture-historical integration; and (3) explanation, or processual interpretation. Particularly important is MacWhite's demonstration that there are logical prerequisites—a point that

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has sometimes been overlooked in the few published syntheses or interpretative efforts that might fall in the "processual interpretation" category. MacWhite points out, for example, that the study of a complex historical problem must follow the simple historical process of tracing the development and diffusion of types, and their interrelationships in time and space.

Hawkes (1954), addressing New World archaeologists, suggests more detailed study of ecology, of raw materials and their sources and diffusions, of primary diffusions or migrations and secondary diffusions or influences. His major points are that we need more complete studies and should give more attention to the methods of interpretation generally included under the direct historical method. This method is the most certain for interpretation of observed archaeological fact, but there are other ways of reconstruction, of interpretation of function, and of studying cultural processes. As I understand them, Hawkes' suggestions fall in the vast realm of "culture-historical integration" rather than in the area of "processual interpretation."

Naroll (1956), presenting a preliminary index of social development demonstrates the validity of a number of concepts and the practical workability of several assumptions that are definitely applicable to problems of archaeological interpretation. His basic assumption is that "... urbanization, occupational specialization, and organizational ramification are the main constituents of social development," and he presents figures documenting the existence of an allometric relationship between such specialization and ramification—as components of urbanization—and population size. Interpreted in archaeological terms, this means that generalizations about social, religious and political structure, and the degree of specialization to be expected, can be proposed on the basis of sheer estimates of population size. His suggested correlation between technological advancement and warfare could also be of considerable importance. The significance of this approach for the archaeological study of the dynamics of social development is obvious, because—a matter to be developed further on—archaeology can usually provide clear, sharp information about population size and, even without sharp problem formulation, has provided evidence about occupational specialization and ramification in terms of political and social structure.

Almost as many differences of opinion are emerging about processual interpretation as there are in potsherd classification. Sound theory is essential, but sound method and completeness cannot be ignored. Unless the levels of observation, description, and interpretation are each unravelled in its necessary turn, complex historical interpretations can only be hypotheses, not suited for further generalization. How can one generalize from—or even test against—the interpretations which were made by the Society for American Archaeology seminar on culture-contact situations (Wauchope 1956), a number of which are based on unstated assumptions about culture succession and contact? Here a really undefined late Southeastern culture known as "Lamar" is attributed to the fusion of the Macon Plateau culture, which is, if not known, at least knowable, with the diffuse "Swift Creek" culture, which actually is a ceramic tradition. All one can really be

sure of is that Lamar ceramics blend styles from the Swift Creek and Mississippian traditions—the latter a widespread and long-lasting tradition of which Macon Plateau is only a single-community representative. Frequent contact between cultures whose ceramics were variants of these two traditions could be predicted from their known distributions or positions in time. Fusion of the ceramic styles could well have proceeded at varying rates at many points. Other instances could be cited of advanced processual interpretation based on little-understood culture or ceramic complexes, in this volume and elsewhere.

In many areas of North America, local ceramic sequences are not well known, and sometimes are almost unknown. If regional interrelationships are not taken into account, the validity of interpretations must be suspect. Efforts to study complex social phenomena must be based on sound observation and accurate culture-historical integration. Assumptions cannot be an acceptable substitute.

Regularities in the processes of cultural development cannot be determined until the cultures are reconstructed in some degree. Reconstructions of form, and sequences of forms, are impossible without the establishment of properly documented, clearly understood units, and their equally clear and well-documented arrangement in space and time. Without this foundation, interpretations at more complex levels are apt to be so erroneous as to invalidate any generalizations based on them.

When North American archaeologists have realistic, factually-based reconstructions of cultures, whose relationships are documented by potsherd and projectile-point chronologies, they will be able to study culture history. It will then be possible to approach the study of cultural regularities and their causative factors, which we share with other branches of anthropology and other social sciences. Phillips (1955: 247) has stated this position very well, as follows:

archaeology in the service of anthropology, concerning itself necessarily with the position of unique events in space and time, has for its ultimate purpose the discovery of regularities that are in a sense spaceless and timeless. And since it appears that a comparative method alone will disclose such regularities, it follows that archaeology is faced with the problem of finding, in the seemingly endless flow of cultural and social events, forms and systems of forms that are not only comparable to each other, but also comparable to, or at least compatible with, the forms and systems of forms of cultural and social anthropology.

Archaeological practice, like that of any other science, is conditioned by the problem-interest and awareness of its practitioners. Rouse (1953: 58) points out that the investigator must choose an all-inclusive program or one with limited objectives, and prefers the latter as more practical. Another factor Rouse notes is what might be labeled the "tradition of practice"; that is, particular objectives, such as establishing chronology or recording architectural forms, which direct archaeological procedures and are a matter of traditional concern in regions or fields of study. Willey, discussing New World archaeological theories and interpretations (1953a: 361), states the case neatly in general terms:

For, as problems are conceived in theory, the attack upon

problems is similarly conceived and methods are selected or forged for this purpose. Likewise, as theory sets up the problem frame of reference, results are inevitably conditioned.

This conditioning is most unfortunate, because of our built-in destruction of our own materials. A great number of mounds, for instance, were excavated early in this century by persons and institutions whose problem-interests were limited to architectural features, the question of pre- or post-Columbian origin of the mounds, or the collection of specimens to be studied, out of context, for some unspecified problem. Concrete evidence applicable to problems of religious form and process contained in such mounds is not now available. Any excavation directed toward limited problems can destroy evidence that could have been used in the solution of more complex problems concerned with form and function. I do not believe that the reverse is true.

Because of the obvious and spectacular nature of the remains, which included preserved buildings of considerable size, work in North America on basic chronicle—the definition of cultural units in minimum terms of artifact typology and the arrangement of these units in time through stratigraphic position and seriation—was undertaken first in the Southwest. Probably for the same reasons, work was first begun there on dwelling-house plans, community patterns, ceremonial structures, and their relationships. With this early start, archaeologists in the Southwest have been, and are, doing more with problems framed in social and religious terms than archaeologists in other areas of the United States.

Still, Southwest, Southeast, Northeast or Pacific Coast, an archaeologist interested in studying social and religious patterns must plan his work accordingly. Some years ago, Taylor (1948) pointed out that North American archaeologists were not getting all that they could from excavations and collections; that they needed analysis, collection, and recording aimed at problems other than the rather sterile end of sheer chronicle. Some of his suggestions and criticisms have taken root, and I expect that more of them will.

On the other hand, awareness of certain problems does affect even observations in the field and the less intricate syntheses. The complex problems involved in the study of historical processes must be kept in mind during field work and descriptive publication. The data permitting reconstruction of cultural forms and processes will then be observed and reported. These reconstructions, whose claim to reality can be documented by reference to observed phenomena, will help eliminate the otherwise inevitable misinterpretation at the processual level. Thus, evidence useful for social and religious reconstruction and interpretation cannot appreciably be derived from an ordinary survey that produces maps and surface collections of sherds; nor can it be derived from strata cuts or test pits with their profiles showing all the structure floors that have been cut through and the sherds salvaged from contexts in the fireplace, under the bed, and outside the back door.

Probably we shall always need the surface or strata-cut sherd-collecting approach. Archaeology designed to attack other problems will not completely substitute for it, any more than the reverse is true. These standard

techniques must be used to obtain cultural time and space dimensions. But now, with general outlines established for many areas, archaeologists can work on cultural problems of the next order, accepting incidental evidence on chronology and distribution as a by-product of their investigations instead of the sole aim. It is possible now, in many areas, to change our point of view and "look at archaeological sites as local social groups instead of as cultures or phases. Cultures are fluctuant, but social groups are clear-cut" (Chang 1958: 324).

The study of prehistoric social and religious patterns needs the foundation provided by traditional archaeology, and knowledge of the contemporary ecology and the kind and degree of adaptation to it are prerequisites for accurate reconstruction and valid interpretation. Understanding of the economic system of the culture being studied is equally necessary. These points, which have been particularly well handled in theory and practice by such archaeologists as Clark, Hawkes, and Childe, need not be discussed further here.

Haury (1956: 3) has pointed out that ethnologists are responsible for most attempts to interpret North American archaeological data in social terms. It was not accidental that Julian Steward, one of the first ethnologists to use archaeological settlement-pattern data, was a prime mover in making the pioneering Virú Valley settlement pattern study such an important part of the Virú project (Willey 1953b: xviii).

In several articles appearing in the *American Anthropologist* during the past few years (Chang 1958; MacWhite 1956; Naroll 1956; Sears 1958), and in the symposium volume, *Prehistoric Settlement Patterns in the New World* (Willey 1956), American archaeologists have recently shown interest in such components of prehistoric cultures as social systems. This literature demonstrates that it is possible to reconstruct social and religious patterns to some degree and that there is some point in doing so.

With the proper approach it should be possible to discover and document a great deal about social systems and the political and religious organization for most prehistoric North American cultures. There must be limits, kinds of information we cannot reconstruct, but until we have tried we shall not know where these limits are.

Each of the different categories of evidence that can be used for the study of prehistoric social systems requires development of its own research techniques at all levels from basic problem formulation to excavation. I use the term *evidence* advisedly. A prehistoric social system cannot be excavated like a house, nor can it be studied with the techniques used to define ceramic complexes or to erect ceramic chronologies. Through excavation and other methods, archaeologists can collect and record evidence that can be used in the study of social systems, and they can develop techniques for such study if their research is oriented toward social problems.

It is not my purpose here to make further general suggestions, but rather to point out several kinds of evidence susceptible of interpretation in terms of social

and religious organization; to indicate the kinds of reconstruction that have been based on this evidence and the interpretations that have been made; and to suggest further lines of inquiry.

Any single class of evidence can yield data for only the most limited social reconstruction, and such reconstructions will have a high probability of error. Inferences and reconstructions from one class of data must be verified and correlated with those based on other classes before any degree of certainty is obtained.

While reconstructions based on historic data may be more accurate than those based on fully prehistoric data, we need not feel as pessimistic as Hawkes (1954: 166-68) about the latter. There are techniques available for the interpretation of prehistoric archaeological data. Willey (1953a: 380, and personal communication) has suggested that there are three categories of analogy, and inference based on the analogies, which can be used in this interpretation. These are:

(1) Analogies drawn from tight historical context, i.e., direct interpretation of a documented site and culture from historic sources.

(2) Analogies drawn from loose historical context such as interpretations of southeastern archaeology from general southeastern ethnology.

(3) Analogies drawn from general comparative data; human-wide analogies, e.g. a sharply pointed stone is a spear point.

Gradations and combinations of approaches are probably necessary for the kinds of evidence that archaeologists must work with. For example, if a prehistoric culture is known to be ancestral to a well-known culture of the historic period, inferences from the culture of the historic group and from loose historical context may be used to supplement each other in reconstruction and interpretation. Another gradation of approach would be the use of inferences from interpretations based on other, better-known prehistoric cultures demonstrably related to or in contact with the subject culture.

SETTLEMENT PATTERNS

... the way in which man disposed himself over the landscape on which he lived. It refers to dwellings, to their arrangement, and to the nature and disposition of other buildings pertaining to community life. These settlements reflect the natural environment, the level of technology on which the builders operated, and various institutions of social interaction and control which the culture maintained. Because settlement patterns are, to a large extent, shaped by widely held cultural needs, they offer a starting point for the functional interpretation of archaeological cultures (Willey 1953b: 1).

I have suggested that settlement patterns can be divided into a number of subclasses, each requiring a different technique for its interpretation (Sears 1956). Chang (1958: 299) has also suggested sub divisions. The ecological dimension suggested by Vogt (1956: 174-75) may be omitted for present purposes, as it would reflect total adjustment to or control of the environment far more than internal social organization. A combination of Chang's and my suggestions then, fostered by a suggestion from Willey (personal communication) emerges as follows:

(1) Settlement pattern: broad, over-all term as defined above, covering

(a) all aspects of community pattern, and

(b) environmental adjustment inherent in or reflected by this patterning.

(2) Community pattern: The strictly social aspects of settlement patterning, including

(a) Site community pattern—internal patterning of single communities; the microcosmic aspect.

(b) Areal community pattern—complex patterning of the communities of a definite culture complex within the definable culture area; the macrocosmic aspect of community patterning.

Obviously, a site pattern can indicate something about the social system of a community. The arrangement of communities and ceremonial centers, in a culture area as definable by conventional studies, could provide information about the organization of a total cultural unit such as a tribe or a confederacy. Here I cannot agree with Chang (1958: 300) that

the economic adjustment of a Neolithic community is the primary determinant of its community patterning. . . . It is reasonable to say that the economic condition of a Neolithic community determines its community type through the medium of land ownership, which controls the patterned manner in which the land is divided and plotted with dwelling and other houses.

Certainly economic adjustment sets limits, but in the Southwest, social factors appear to have been dominant in site community patterning (Haury 1956: 6; Reed 1956: 16-17; Wendorf 1956: 8-20). In the populous societies of the late prehistoric Southeast, religious factors, which seem to have been particularly important in social organization, were both direct and indirect determinants of the site patterns in major ceremonial centers (Sears 1956: 94).

Willey (1956: 1) has denied the existence of a "settlement pattern approach," implying that settlement patterns should rather be considered a class of data available for analysis and application to many problems through a variety of approaches. His point is well illustrated by the symposium volume on settlement patterns (Willey 1956), in which articles range from outlines limited to general statements about settlement size and type in various culture periods, with some description of physiography, to efforts to use settlement pattern to reconstruct and interpret prehistoric social and political organizations and the causes and effects of changes observed in them through time. The articles by Haury, Reed, and Wendorf provide, in rather speculative fashion, very good examples of the potential use of settlement patterns for the study of prehistoric social systems. Supplementing data on excavated structures and on site and areal patterns with considerable knowledge of the relevant historic cultures, these writers outlined long-term changes in the social and political systems of the three major cultures—Anasazi, Hohokam, and Mogollon—of the southwestern area. For these cultures, each utilizing and adapted to a different variant of the general southwestern environment, they provide testable hypotheses concerning both the nature of the social and political systems and on their changes

through time. It is quite clear that social and religious organization were dominant factors in the determination of site community patterns. Wendorf (1956: 24) points out that isolated Great Kivas—subterranean ceremonial structures in the Anasazi and Mogollon areas—demonstrate

the ceremonial integration of otherwise discrete units and the enlarging of the community consciousness through an already established ceremonial structure.

Similar guides to comprehension of the social-religious significance of areal community patterns may well be found elsewhere, and could eventually be of considerable value. The speculations in these articles are tightly tied to the pertinent concrete data, and thus provide testable hypotheses for further work. William Ritchie's settlement-patterns study in New York state and the writer's similar study on the Gulf Coastal Plain are planned to produce such information. More such projects in areas other than the Southwest have been or will be initiated, I am sure.

Martin and Rinaldo (1950) have used community-pattern data in a provocative and sound effort to reconstruct the prehistoric social organization of the southwestern Mogollon culture. Dwelling and room size, correlated with the size of the total settlement, and supported by a study of changes and distribution of metates and other artifacts, provided information about family size and type, and the social structure of the community. The inferences developed from these reconstructions, aided by analogies with historic cultures known in some detail, are quite explicit and indicate substantial change through time, as well as the social processes produced by and causing the changes. In their conclusions Martin and Rinaldo state: "These are assumptions and nothing more. We do not think of them as provable or proven. We suggest them as possibilities" (1950: 569). They point out that more data would allow further reconstruction with a higher degree of probability.

The most systematic effort to interpret settlement-pattern data in socio-political terms is Chang's article on Neolithic social groupings (1958). A cross-cultural survey, and precise classification and definition based on ethnographic fact, indicate a high degree of correlation between community plan and lineage structure, each simplified and condensed into a number of classes. The most positive correlations, those between small planned villages and single lineage organization, and between segmented villages and multi lineage social organization, hold real promise for the interpretation of archaeological sites as communities. I think the ethnographic samples need expansion, refinement, and correlation with ecological potential, and that aggregates of communities—areal community patterns—need careful attention.

Settlement-pattern interpretation thus makes possible many inferences about social organization. House and room size, room plan, type and placement of special-purpose structures in the site, over-all site plan, and areal settlement patterns are all classes of evidence that have been interpreted in terms of kinship structure, social organization, and religious and political organization.

CEREMONIAL STRUCTURES

The category of ceremonial structures includes many physically and functionally different kinds of structures. The unifying factor is known or assumed in the ceremonies of the culture with which they were associated. Sometimes this interpretation is based on the probability that construction, and therefore usage, of the structures was communal, and the knowledge that communal activities in North American Indian cultures were usually associated with the ceremonial aspects of religious practices. An incomplete listing includes temple, burial, and other kinds of mounds; earthworks, such as sacred circles; plazas; kivas; and ball courts.

These are a much-neglected source of data. Religious and political systems were intimately related in all North American cultures sufficiently advanced to have produced ceremonial structures. Data on the ceremonial activities, and the religious and political systems of these prehistoric cultures is most likely to have been "fossilized" in structures built for, during, or as a by-product of, major ceremonies. As I have attempted to show (Sears 1954, 1958), excavation and analysis of a mound as a fossilized ceremony rather than as a repository for pots, sherds, and bones, will allow at least partial reconstruction of the ceremonialism and interpretation of it in social terms.

The most important evidence is provided by the sequence of construction and deposition, and the association of such varied elements as types of soil, copper plates, and pots. I do not believe that the archaeologist can observe and record all of the data on construction sequence, and on the function of each element in the mound and in the ceremonies attending its construction, unless he excavated the structure with the problem of ceremonial reconstruction firmly in mind. If he cannot reconstruct the ceremonial sequence when he finishes excavation, I doubt that more than a small part of the potential reconstruction will ever be possible from the usual meticulously-kept records of artifact and burial location, the scale drawings and photographs, and analysis of the specimens which are removed. There are always a great many phenomena that could be observed and recorded. A choice is made, finally, between alternate and even mutually exclusive possibilities in technique of excavation and method of recording. Presuming that a compatible system can be worked out, the final choice of how to excavate and how to record should be made on a functional basis. Methods could be developed permitting determination of the function of a layer, a pit, or a structure, rather than merely its vertical and horizontal position as a haven for classifiable artifacts. A record of where a layer is may be adequate for purposes of chronicle; documentation of what it was is a prerequisite for reconstruction and interpretation.

There have been a few interpretations in social terms of the mounds and earthworks in Eastern United States, and of the cultures associated with them. Spaulding (1952) and Webb (Webb and Snow 1945: 314-317), for instance, have presented evidence for a strongly class-structured social organization in the Adena culture, the

earliest of the midwestern mound-building cultures. Spaulding has used this inferred social organization as data in a paper discussing the origins of Adena. Here the evidence is quite explicit, and the inferences are tightly related to the data. Deuel has rather broadly reconstructed the social and religious organization of the Hopewell culture, responsible for many of the great mounds and earthworks of the Ohio and Illinois area, and for such widely known ceremonial artifacts as effigy pipes, copper head-dresses, and obsidian blades. He has drawn analogies between Hopewell society in the Midwest, around the beginning of the Christian era, and the society of the 18th century Natchez Indians of the lower Mississippi valley (1952: 54-55). Deuel's reconstructions are probably valid; his text generally lacks specific references, comparisons, and documentation; and the reconstructions are not tightly related to the evidence from which they are presumably drawn. Rather, they are based on Deuel's unstated knowledge of Hopewell materials—a type of interpretation referred to again below.

One of the latest culture types or horizons in Eastern United States, covering a large area including most of the Midwest and Southeast, is known as Middle Mississippi. Temple mounds—huge truncated earthen pyramids—dominate many of the sites of this cultural unit. Statements that Middle Mississippi society was highly organized because the construction of the temple mounds required a large, well-organized laboring force have been made so often that specific references would be needless. A study of the forms taken by this organization remains for the future.

In the Southwest, the general functions of the ball courts, and the underground ceremonial chambers called kivas, are well known from recent and contemporary usage. This knowledge has been used in the interpretation of every site and culture where they occur archaeologically. Attempts at understanding the basis for their different shapes, locations, and interior arrangements have been made. The articles on the southwestern area in the settlement-patterns symposium volume (Willey 1956) again offer the best examples of inference from these sorts of data.

It has been possible to demonstrate in single burial mounds the existence of social classes in certain prehistoric North American societies. This class differentiation, conceivably restricted to ceremonial contexts only, might not have been the norm for everyday activities. Further work on these structures will show whether or not the features and associations indicating this social organization are repeated; and the evidence can be cross-checked with reconstructions based on other information. If more complete studies, and especially comparative studies, indicate that this social stratification is limited to ceremonial contexts, we shall at least have made a beginning in comprehension of religious systems.

BURIALS AND GRAVE GOODS

Reconstruction of ceremonies on the basis of a single burial does not seem possible unless the grave includes artifacts or is in a complex structure such as a mound. The interpretation thus depends primarily on artifactal

or structural data which may allow reconstruction of the mortuary ceremonies, and perhaps on generalizations about religion and the social order. Therefore the categories of ceremonial structures and burials *per se* should be conceptually separated, no matter how difficult this may be in practice.

A very successful reconstruction of an early, widespread burial cult, based on burial practices, grave goods, some less tangible items, and their associations, has been effected by Ritchie on the basis of what were essentially single burials (1955: 61).

Ritchie's analysis of everything from human ash to charred leather, and his observations of artifact context, with sequences of events worked out from context and association, demonstrate clearly that, with attention to such problems, procedures can be reconstructed in considerable detail. He also shows that the reconstructed procedures can and should be tightly related to the physical evidence. The interpretation of these reconstructed procedures is a second level of activity. It results in a set of inferences, with a somewhat lesser degree of probability, whose validity may be measured or evaluated in some degree by the concreteness of the analogies used. Ritchie has carefully differentiated between procedural inferences and interpretation of the cultural ideologies underlying them.

With multiple burials or burials in mounds with large quantities of associated specialized artifacts—usually Hopewell burial mounds or those of the Gulf Coastal Plain—it has often been suggested that some one of the burials was that of a person of superior status. In one of the most obvious cases, this interpretation has been taken out of context and included in a very generalized, sketchily documented interpretative section in the conclusions to a much larger work (Deuel 1952: 263-65). Interpretation here would have had considerably greater validity and utility if the procedures of a specific burial were reconstructed first, and then used to reconstruct a specific ceremony.

Webb (1959), excavating the Belcher mound, noted evidence in the form of relative position of bodies in graves, and in locations and types of artifacts, which rather clearly indicates social differentiation of the persons buried in each grave. He has also been able to relate the graves to specific structures on various superimposed platform-mound surfaces. The possible and testable inferences concerning social and religious structure at this specific site, and in the pit graves with multiple primary burials which are characteristic of the culture of the Caddoan area, west of the Mississippi River and south of the plains, are obvious.

Krieger has demonstrated that social and prestige factors are involved in the internal arrangement of individual mass graves in the Caddoan area, and shows that the differences in sex and age distribution between such graves in various sites could reflect other such factors (Newell and Krieger 1949: 206). Clarence B. Moore, recording the type and location of burials and artifacts in the large Caddoan pit-graves, also documented different placements of pits with varying contents and arrangements in at least one case. He interpreted this as indicative of differences in importance of graves as units, and therefore of the paramount individual in each (1912: 561).

These reconstructions stop considerably short of ceremonial, let alone social reconstruction and interpretation. Regardless of the number of bodies or the type and quantity of grave goods, a burial can only indicate the probability that some one person in a given culture at a given time was ascribed superior social status. It cannot tell, by itself, the sort of social or political-religious system of which this status was a part. Such reconstruction can only come from joint analysis of several lines of evidence.

SPECIALIZATION IN ARTIFACT MANUFACTURE

A few classes of artifacts, usually found in similar cultural contexts, are clearly the work of particularly skilled artisans. The quality of the workmanship, indicating complete mastery of the craft, demonstrates the existence of specialized craftsmen.

This is perhaps a minor category of evidence, with which very little has been done in North America. Yet it should be given consideration since the extent to which a society can afford to maintain specialized craftsmen is a reflection of its wealth and organization. The degree of technical competence shown in certain classes of artifacts is then social data.

A pertinent example here might be the specialized pottery of the prehistoric Weeden Island culture, found in many burial mounds on the Florida Gulf Coast and adjacent areas. This ware, which often occurs in effigy and unique forms, sometimes with perforations as part of the decoration, was probably used in the religious and ceremonial areas of cultural activity, not only in the mortuary ceremonies. This interpretation depends, in part, on the interpretation of mortuary ceremonies in this and other cultures as a reflection of the ceremonial aspects of social organization (Sears 1958). The effigy and non-utilitarian forms of this ware were executed with such technical virtuosity that its existence in considerable quantities virtually demonstrates that there was a class of trained artisans, who were supported by their societies during its manufacture. These artisans, then, form a distinguishable social class.

Similar command of technique and materials is also shown by such artifacts as the effigy pipes and copper ornaments of the Ohio and Illinois Hopewell culture; the *répoussé* copper work, shell engraving, and other products considered representative of the very widespread, late prehistoric religious manifestation known as the "Southern Cult"; and some of the fine engraved pottery of the Caddoan area. Comparable artifact classes, suggesting a class of specialists, occur in other archaeological cultures of the eastern United States. If analysis demonstrates that some of these artifacts were distributed from a single source, then the evidence for the existence of full-time specialists in certain communities becomes more certain, and the existence of a network of regional social relationships is indicated.

ARTISTIC REPRESENTATION

Most of the North American artistic representations that might be considered here were produced by the specialized craftsmen discussed above. The preceding section used these items as possible evidence for the existence of a class of artisans. Here, costumes, paraphernalia, and activities represented are regarded as in-

dividual items of evidence, each potentially leading to some limited reconstruction of ceremonial activities.

Analyses of these art forms, directed toward ceremonial reconstruction, could lead not only to understanding of the ceremonies themselves and the uses of each bit of costume and each object, but in a further step might also lead to reconstruction of some of the religious concepts involved. Such analyses have been few, and are usually limited to casual interjections in texts concerned with other problems. The only real exception I know is the analytical section of the report on the Awatovi Kiva murals (Smith 1952).

Rather explicit reconstructions of Southern Cult ceremonies in their various constituent parts may be possible through the detailed representations of reasonably uniform costumes, paraphernalia, and activities. Fenton (1953: 204) has noted the resemblance of Cult eagle costumes and those of Pueblo eagle-dancers. An important element in Iroquois Eagle Dances is a striped forked pole. A striped forked pole or staff, apparently a snake symbol, is an important element in representations of ceremonies engraved on conch shells found at the Spiro site (Hamilton 1952). Is it not at least possible that comparative analyses of cult representations and artifacts, Iroquois Eagle Dance activities and paraphernalia, and Pueblo Eagle Dance costumes, activities, and paraphernalia might lead to some understanding of the form and content of the ceremonies, and the structure and content behind them, depicted in Southern Cult art?

Certainly until an archaeologist who knows the materials well works with ethnological sources of the 16th century and later, or an ethnologist familiar with the ceremonial life of Indian groups pays attention to these representations and regalia, we cannot say that the costumes, activities, and equipment are incomprehensible, or that they are not specific leads to definable ceremonies and perhaps to a high degree of religious reconstruction.

SUMMARY

The reconstruction of prehistoric social and religious systems and interpretation of their cultural meanings and significance will finally lead us to study of their processes of change and their transmission through time and space. This latter, just becoming visible as a possibility, is, I think, the most important single goal.

Procedures will inevitably involve step-by-step reconstruction, starting with the various ceremonies and synthesizing the religious systems from them; beginning with a probable social system for a single community and synthesizing political systems from these. The religious systems and the socio-political systems of broader cultural units such as those that may be hypothesized on the basis of single large ceremonial centers for such currently identifiable phases as Etowah and Moundville, may then be reconstructed. In actual practice, it is to be expected that inferences made on the basis of ceremonialism and religion will refer back directly to social data and inferences. Study should begin with con-

ceptually discrete segments of the over-all religious and political systems. But, since these were real cultures, full comprehension, reconstruction, and interpretation will develop from and into an interlocking web.

Possible approaches to this kind of study have begun in several areas of North America. The study of settlement patterns has led to reconstruction of social systems in general, but well-documented, terms, and to an understanding of the relationship between community plan and lineage organization. Martin and Rinaldo have made rather specific inferences about prehistoric Mogollon family size, type, and social organization, as well as a study of change through time in these categories, so explicitly related to the concrete evidence that it facilitates further work on their inferences.

Ceremonial structures, and particularly burial mounds, have provided data bearing on social organization in terms of class system and the reconstruction of ceremonies. Ceremonial structures generally, as products of massive social efforts, have often, if not explicitly, served to demonstrate the close relationships between socio-political systems and ceremonial-religious organization.

Individual burials have provided information on social differentiation. This scattered type of evidence, difficult as it may be to handle, has been fully exploited by Ritchie in the reconstruction of a prehistoric burial cult, with which he offers currently valid interpretations of its historical and cultural significance.

In areas of specialization and artistic representation, only suggestions have been made, such as Fenton's comment about Eagle Dances and costumes. Very broad generalizations have been produced in a somewhat greater number of instances.

WHAT HAS BEEN DONE?

A number of broad interpretative essays and cultural reconstructions have been produced. Actually, these are of two sorts, although the gradations are not at all clear.

The nature of the first sort is pointed out by Deuel's wish for statements "... readily understandable to interested laymen and fellow writers in adjacent areas" (Deuel 1952: 11), and illustrated by Lewis and Kneberg's recent book for "... students, for amateur archaeologists, and for all other persons with curiosity about the Indians" (1958: v). Free-swinging reconstructions for this audience are certainly needed, and those offered by Deuel, Lewis and Kneberg, and a few others serve this need very well. Detailed documentation is definitely not desirable here, nor is it compatible with popular writing. The intelligent public wants to know about prehistoric cultures, not potsherds, and will not fight its way through the impediments of technical jargon, references, footnotes, and so forth. The only documentation needed is the writer's knowledge and experience. Surely professionals can judge, presuming prior technical publication, the soundness of the reconstructions.

On the other hand, technical reports and journal articles, presumably in a bow to supposed professional trends, are sometimes written in much the same way, and actually present some of the wildest reconstructions. The reader is suddenly confronted with a wholly imaginative detail that could not conceivably be substantiated from any known archaeological data with

any currently available techniques of reconstruction or interpretation.

There is a place for loose, informal reconstructions—although even here with imagination used for extrapolation only from documentable facts—for a popular audience. There is also room for carefully-reasoned, researched, and fully documented reconstruction and interpretation. This is the area of work in the social sciences which archaeologists are only now beginning to exploit in North America.

WHAT CAN WE DO?

I have tried to touch on some of the kinds of evidence that have been used in reconstruction and interpretation of social and religious structures and processes. In each case some of the utilizations of these resources to date were pointed out. Here I shall reverse this procedure, and attempt a statement in terms of potential product instead of potential raw material.

Social Structure

Family size and type: inferred from room size and type; number of hearths and their location; and the arrangement of domestic furnishings. Single dwellings would be handled in the same way as rooms, with hearths and furniture providing the same sort of qualifying and corroborative information. Agglutination of individual units into larger units as complex buildings and/or communities may then be considered, leading to reconstruction of inter family relationships and organization, regardless of the type of family unit indicated.

Lineage Structure: close correlation between types of site community pattern and basic lineage organization has been documented.

Class Structure: inferred from differences in grave goods and types and from total reconstruction of mortuary and other ceremonies. Artistic representation, with evidence for restricted use of paraphernalia, sumptuary differences, and ceremonial activities can be important here. Evidence for trained specialists, full time or not, is available in evaluations of some artifact classes.

Religious Structure

Ceremonies: reconstruction is possible from data provided by burial mounds, provided that the sequence of events, burial types and accompaniments, and artifacts are all recovered and are cross-correlated. Artistic representations may give leads to actual activities; certainly to costume and paraphernalia. Site community patterns, area community patterns, or both can lead to some understanding of ceremonial pattern and its cultural importance in terms of inter- and intra-community participation.

Religious and Cultural Patterns

Ceremonial structures, and their spatial organization in a site and in a definable culture area, analyzed in terms of reconstructed ceremonies, should permit valid inferences as to religious patterning above the level of specific ceremonies. Inferences about class structure will probably, in most North American communities, be inferences regarding the structuring of the religious organization. If evidence for the existence of secular political authorities, distinct from religious functionaries, should appear, then the problems will of course become more complex.

I find it difficult, at an abstract level, to carry this outline further. It does seem possible to excavate an

archaeological site with the kinds of problems and the sorts of applicable evidence discussed in the foregoing in mind. This done, the brief outline above, if valid, indicates the probability of a great deal of reconstruction and interpretation of social and religious systems. Although it is highly improbable that study of either of these aspects of a primitive society can be carried very far independently, the effort should be made so that inferences can be cross-checked.

With whatever reconstructions of social and religious structures are available, the student should be able to move to a study of the *hows* and *whys* of cultural process. If he knows something about the *what* of a particular religious manifestation, he is in a position, never attainable through sherd analyses, to work on the *whys* of its intercultural spread.

The classes of evidence and classes of cultural reconstructions and interpretations referred to here are not only interrelated, but their study cannot proceed without conventional studies establishing the bounds of cultural units in time and space. Just as importantly, they must be accompanied by studies of the prehistoric ecology and adaptation to it, and studies of the economic

systems of the various cultures. I think the need for more ethnohistorical studies will soon become manifest. These will permit us to use the direct historical method more often, and will give us a greater stock of comparative materials, such as the identification of historic peripheral survivals, for reconstruction of prehistoric social and ceremonial systems.

Limitations will always be with us. Perhaps our archaeological balance sheet will remain in the red, with imponderable liabilities continuing to outweigh exploitable assets. One factor here is that how much we can do in the study of social and religious systems, quantitatively, depends on how much there was. If a prehistoric culture had an economy based on hunting and gathering, in an environment with a low potentiality for that type of exploitation, we certainly know that there could not have been a very complex social and political structure. Generally, this means that the later periods, in the East or the West, are those about which we have the most to learn, and those in which the studies are and will be of greatest complexity.

Comments

By T. N. CAMPBELL*

Sears presents a strong argument for carefully documented reconstructions as the principal means for arriving at meaningful statements about social and cultural forms and processes. Having attempted such reconstructions himself, he is aware of the difficulties involved and appears to have no illusions about easy and rapid success. For those who are inclined to be sceptical or defeatist, he points out that while there undoubtedly are limits to cultural and social reconstruction, these have not yet been defined. I find his argument convincing; his discussion timely, stimulating, and challenging.

In this paper, Sears is primarily concerned with the reconstruction of social and religious systems in the more complex cultures of North America. It was somewhat disappointing to find that he ignored the hunting and gathering cultures that were widely distributed over North America in the more remote past (Paleo-Indian and Archaic stages), and in many areas survived into the historic period. Unintentionally, I think, Sears leaves the impression that little reconstruction can be done with cultures that were not based on agriculture. This is of course debatable, and we might quote his own words: "Until we have tried we shall not know."

If archaeologists are to devote more effort to the reconstruction of social and religious systems, I believe we shall need more help from ethnologists than we have been receiving. Or we might insist that the training of archaeologists

include some ethnological field experience. Good cultural reconstructions demand not only special abilities, but also a high order of anthropological sophistication.

And if we are to move nearer to the goals designated by Sears, it seems to me that our strategy must be modified. Smaller archaeological projects would be much more effective if they were coordinated. There should be many more large-scale, carefully planned, long-term projects that permit a more leisurely pace in excavation and analysis of the data. The kinds of evidence essential to the success of Sears' program are not readily obtainable when work is carried out under pressure. This is one of the major deficiencies of emergency excavation, such as the highway and river basin salvage archaeology that now absorbs much effort in the United States. These mundane matters are not discussed by Sears, but they are important and should receive more attention from leaders in the field of North American archaeology.

Sears stresses the importance of excavation with greater awareness of broader problems, and believes that this will lead to the accumulation of more and better evidence for use in reconstructions. I think that similar results may also be obtained through better use of ethnological data. For example, why not select a site or series of sites known to have been occupied in the early historic period by a people—e.g. the Choctaw of the southeastern United States—whose culture, although no longer functioning, is fairly well described in written documents. Instead of following the usual sequence of ex-

cavating and then looking for interpretative aids in the documents, the latter would first be analyzed by a team of ethnologists and archaeologists attempting to formulate, as precisely as possible, the social and religious patterns of the Choctaw as revealed in the surviving documents. This team would then try to predict how these patterns would be expressed in the archaeological sites, making allowances for the preservation factor, acculturation, and a certain amount of natural and human disturbance. Excavation would follow, with the archaeologists fully alerted to the potentialities. With some luck in the choice of sites, this unconventional approach ought to indicate something about the limits and validity of strictly archaeological reconstructions. I am certain that it would sharpen field observations by archaeologists.

By THORNE DEUEL*

Sears has mapped out, logically and clearly, the road by which the archaeologist must travel in order to reconstruct the social and religious customs of prehistoric preliterate peoples. Students of prehistory can find no serious faults with the methods he outlines. My comments are not a criticism, but a suggestion that the sources from which evidence for prehistoric reconstructions may be drawn be expanded to include study of the cultures of extant primitive¹ peoples, and the comparison of these cultures with each other and with

* ¹ Primitive as used here refers to peoples having either a hunting-collecting economy or a simple food-raising economy employing only human energy for labor.

those known only through archaeological remains.

Culture involves environmental, psychological, and cultural factors of which, especially in the social and religious fields, archaeology yields only secondary or more remote evidence. Living primitive peoples are, of course, the province of the ethnologist rather than the archaeologist. I venture to say that the ethnologist is more apt to discover, from the more complete evidence available to him, the principles of cultural stability and change. (Presumably most anthropologists believe that cultural laws exist, although as yet undiscovered.)

The best apparent method for bridging the gap between living and archaeological primitive cultures is by way of material objects and the economy, population density, and maximum settlement size, as suggested by Naroll (1956). Analogous studies of existing ethnological cultures, technologically simple and complex, can very well provide a means by which cultures of the same order can be compared, and thus pave the way for the reconstruction of prehistoric societies.

By DON W. DRAGOO*

Sears' article reflects the growing recognition by many North American archaeologists that their endeavors should not be confined entirely to the study of potsherds and other discrete manifestations of culture, but should be directed toward a higher "processual interpretation" of archaeological remains. To some extent, this attitude may be attributed to increased contact with anthropologists whose research has concerned the interpretation of the socio-cultural realities of living groups. The association has generally been a fruitful one, from which both archaeologists and social-cultural anthropologists have benefited.

Today, professional archaeologists in the United States are usually trained in departments of anthropology, initially as anthropologists and secondarily as archaeologists. Probably most of them now consider themselves anthropologists who use archaeological methods to discover the contents, processes, and causes of cultural and social forms in the past and their relationships to extant forms. Some archaeologists, wishing to forget the papers and monographs emphasizing the description of artifact types and chronology, substitute a lot of verbiage in the name of high intellectual interpretation. Sears cannot be associated with these individuals, for he obviously realizes the great value of typological and chronological studies that lead to elucidation of cultural units in time and space, while he

rightly decries the frequent undertaking of these studies as ends in themselves rather than as an integrated step in the over-all understanding of a cultural complex through the methods and techniques of archaeology.

The current emphasis on interpretation is not new in North American archaeology. Many of the early archaeologists, whose methods and techniques were so crude that speculation was all that was possible, published reports that were filled with interpretations to the point of excluding the raw data validating their often wild speculations. With the improvement of methods, techniques, and concepts came a period in which the archaeologists were content to look for patterned relationships, pursued through descriptive taxonomy combined with studies of temporal and spatial distribution. Descriptive and classificatory reports were the order of the day. These practices did not come about through accident, but were determined by the underlying theories and concepts that mold the purpose of archaeology and the objectives of individual archaeologists. To understand the present stress on interpretive studies, therefore, we must take a closer look at the theoretical foundations of archaeology.

The Objectives of Archaeology

Although tracing the development of archaeology as a separate discipline is not necessary here—Taylor's *Study of Archeology* (1948) includes an adequate outline—the purpose and objectives of archaeology, and its relations to other disciplines, are pertinent.

Wissler (1938: xv) defined anthropology as the "specific science of man," of which ethnology, somatology, and archaeology were only divisions, or convenient groups of problems. Herskovits (1948: 3) stated that "in addition to the study of man's physical type and his cultural behavior, anthropology includes prehistoric archeology," and he considered the prime duty of the archaeologist to be that of throwing light on the development of man prior to the invention of writing. Vaillant (1930: 9) took a similar position, suggesting that "archeology has two chief aims, the reconstruction of the life of the people in the past and the arrangement of this life into an historical development." Most views of archaeology share a belief that its major emphasis should be on historical reconstruction.

Linton (1945: 9), however, held that while the immediate and obvious purpose of archaeological work is to increase man's knowledge of his past, its ultimate goal is understanding the processes involved in the growth, flowering, and collapse of civilization, and

the factors responsible for these processes. Strong (1935: 6) considered archaeology and ethnology "two inseparable means to an essential end"—the most complete understanding of human culture at all times and places. This interest in goals other than history is also stressed in a recent definition of archaeology by Phillips and Willey (1953: 616).

The ultimate objective of archeology is the creation of an image of life within the limits of the residue that is available from the past. The procedural objectives toward such a goal may be dichotomized into reconstructions of space-time relationships, on the one hand, and contextual relationships on the other.

Evidently archaeology has two separate but related objectives: (1) the systemization of the space and time data which form the foundations of chronological and distributional studies; (2) the study of contextual and processual aspects of culture. Taylor (1948) referred to the first as "chronicle" and to the second as "historiography." Willey (1953a: 363), who termed them "historical" and "processual," determined three ascending levels of interpretative complexity for each. Until recently, archaeologists in the United States have tended to emphasize historical rather than processual objectives, and basic archaeological problems were framed in accordance with this emphasis.

The Positivist Orientation of North American Archaeology

The basic philosophical assumption underlying the work of American archaeologists is often overlooked. A survey of current literature fails to find any explicit mention of this philosophical foundation; either it is unrecognized, or many archaeologists are fearful of being tagged "philosophers." Archaeology is considered a subdivision of anthropology. As such, it has been influenced by the philosophical orientation of anthropology, which in the United States was strongly affected by Franz Boas and his students. The philosophical assumptions dominating Boas' work (and also the physical sciences) were supplied by Positivism as exemplified by August Comte. Basic to Positivism was the conception of a natural history of mankind, subject to laws of growth comparable to those of physics. Through the collection of large masses of data, the laws governing culture change would in time become evident. In the following statement, Boas (1938: 3) implied his belief that laws can be formulated:

When these data are assembled the question arises whether they present an orderly picture, or whether history proceeds hap-

hazardly; in other words, whether an orthogenetic development of human forms may be discovered and whether a regular sequence of stages of historical development may be recognized. If this were true, definite laws governing historical sequences could be formulated.

The general failure of American archaeologists to engage in theoretical and interpretative discussions has been due, not to the lack of a philosophical orientation, but rather to a Positivistic orientation that has proved unsatisfactory. The laws of a "natural history of mankind" have not become evident from the great mass of data that was collected, although many archaeologists are still hopefully awaiting their appearance. It is a healthy sign that some workers have become uncomfortable and are seeking to put archaeology's theoretical cards on the table for closer scrutiny. Taylor's (1948: 93) belief that the "archaeologists have accepted the admonition to do and die without the incumbence of reason why" is based on their confusion of archaeology's relationship to history and cultural anthropology.

Culture Theory and Archaeological Methods

Most approaches to the study of culture involve either a realistic or an idealistic perspective. Culture, as conceived by the realist, is inseparable from the life of human beings in society, and therefore can have no existence independent of the group to which it is attributed (Bidney 1953: 24). Culture is an attribute of human social behavior, and can be defined in terms of acquired habits, customs, and institutions. Although culture is usually identified as the "standardized social procedures" of a group, with the individual referred to only indirectly as one affected by the customs of the group, some anthropologists, such as Boas and Sapir, stressed the role of the individual in the cultural process. To the realist, the cultural heritage consists of material artifacts as well as non-material ideas, institutions, customs, and ideals.

The idealist's view of culture was separated by Bidney (1953: 24) into three categories of idealism: *subjective*, *objective*, and *conceptual*. The "subjective idealist" conceives culture as the aggregate of ideas in the minds of individuals; Culture comprises the communicated ideas concerning the manufacture of tools, and not the particular artifacts that exemplify the ideas. The "conceptual idealist" looks upon culture as "patterns" of behavior or "designs" for living; Culture is a conceptual "construct," and is abstracted from the non-cultural behavior exemplifying it. To the "objective idealist,"

culture is a "superorganic" flow of ideas, and any particular culture is an abstraction from the historical complex of ideational traditions; and these ideas have a transcendental reality independent of the individuals or societies which hold them. The concept that culture is something conceived by minds is common to all idealistic positions.

Taylor (1948: 100-03) defined culture as a mental construct that pertains to minds rather than to material objects or observable behavior. He based his contention that culture is a mental phenomenon on two premises:

(1) The extended infancy of humans facilitates the acquisition of mental constructs during the long period of parental dependency. Objects and behavior are of no value to the infant; only his ideas pertaining to them are important.

(2) The cultural content is a cumulative heritage of the past, consisting of mental constructs pertinent to various patterns of behavior, to which most of the content of any culture owes its form.

Culture, for Taylor, consists of ideas, which include such categories as attitudes, meanings, sentiments, feelings, values, goals, purposes, interests, knowledge, beliefs, relationships, and associations. The ideas are not themselves observable, but they are externalized through the "action-systems" of the body in the form of behavior that can be observable. This behavior results in the material objects, such as tools, and non-material manifestations, such as styles of graphic representations and dance patterns. The idea is the culture trait; the material object is only the objectification of the trait. Consequently, the true traits of culture are unobservable and can only be inferred from their objectification. Taylor considers the term "material culture," with which the archaeologist must generally work, a misnomer because it relates only to observable results and not to culture itself. From the archaeologist's point of view, culture becomes completely inferential since all his data come from the material results of cultural behavior.

Taylor's idealistic concept of culture is evident in his discussion of archaeological typologies (1948: 113). An archaeological "type" became the "abstract least-common-denominator, median, mode, or mean of a series of empirically similar and associated artifacts." It is through the abstraction of types that the ideas which were in the minds of the makers may be inferred. The common attributes of a group of specimens provide data for an interpretation of their makers' ideas and conceptualizations. This concept of "type"

follows closely that of A. D. Krieger who also held that an archaeological type is a group of specimens that have demonstrable historical meaning in terms of behavior patterns. Any group that Krieger labels a type "must embrace material which can be shown to consist of individual variations in the execution of a definite constructional idea." (1944: 272). For Krieger therefore, a type is an abstraction that provides an "organizational tool" by which the archaeologist may infer the idea of which the material object is only the expression.

James Ford (1954) spoke of the cultural type as an abstraction and "a reflection of the boundaries to one stream of ideas which the cultural bearers considered related." For Ford, types are separable and look "natural" because the archaeologist's sample is spotty and represents a short span of culture history in a certain locality. When the gaps are filled so that history may be viewed as a continuum through time and space, the overlapping of types will render the typology a "meaningless conglomeration." In a companion article, Julian Steward (1954) generally agreed with Ford's theoretical concept of types, but suggested stressing their functional aspects as well as their morphological attributes.

In contrast to the idealistic views of Taylor, Krieger, and Ford, which envisage culture change as a continuous "stream of ideas" that can be segmented into arbitrary types at the convenience of the archaeologist, Rouse (1939) regarded types as once-existent realities in prehistoric cultures. In this view, the typological task becomes that of recognizing these existent entities. A. C. Spaulding's (1953: 305) assumption that cultural types "exist" in culture and may be "discovered" by the use of proper methods, especially by statistical analyses, agrees with Rouse's view that types are cultural realities.

Willey (1953a) has outlined the shortcomings of these two opposed concepts of types. In his view the idealists' arbitrary segmentation of the "stream of culture change" tends to overlook factors accelerating or decelerating the rate of change, and minimizes sudden changes that may result from the impact of external influences on the culture. When types are conceived as cultural realities, certain styles or patterns in the manufacture of artifacts are often overemphasized, while others may be slighted. Willey believes that both conceptions have merit, and suggests (p. 368) that

cultures can be plotted as a dynamic flow, and, at the same time it can be kept

in mind that prehistoric artisans were aiming at modalities which to them seemed fixed and which, undoubtedly, did not change at a set rate of speed.

A paper co-authored by Phillips and Willey (1953: 616) expresses the view that all types possess some degree of correspondence to cultural 'reality' and that increase of such correspondence must be the constant aim of typology.

Superorganic Concepts of Culture

Certain idealists, following the tradition of Plato and Hegel, adhered to what Bidney called "objective idealism," and maintained that the "social heritage" is a "superorganic" stream of ideas, while a particular culture is an abstraction from the historical complex of ideational traditions. Culture was regarded as a heritage of ideas that have a transcendent reality independent of the individuals and societies which carry them.

The term "superorganic evolution" was coined by Herbert Spencer, who conceived it as a process that supervened upon organic evolution. Spencer held that the processes and products of the co-ordinated actions of many individuals could be distinguished from the organic, but could not exist independently. The superorganic products of the action and reaction of societies consisted of material objects, customs, laws, art, science, philosophies, etc. Spencer looked at society "as if" it were an organism subject to its own laws of evolutionary development, but unlike later workers, he never conceived the superorganic as transcending the organic.

Among recent anthropologists, A. L. Kroeber and Leslie A. White have been the most influential exponents of superorganic culture evolution. Kroeber (1917) used the term "superorganic" to designate the non-organic and that which transcends the organic; prior to 1948, he maintained that cultural phenomena were "superpsychic," and that their development and evolution were independent of psychobiological organic evolution. Culture did not comprise the "mental action" of interacting individuals, but only the "body or stream of ideas" existing independently of individual minds. Thus, man becomes the carrier of culture, but not its originator or source. White's (1949) concept of culture, like Kroeber's, also transcends the organic, but differs in regarding technology and the material conditions of social life as the determining factors in the evolution of culture. Culture, when viewed as a reality *sui generis*, is subject to laws of evolutionary development through fixed, predetermined stages.

Kroeber's and White's superorganic views of culture found their way into the theoretical orientation of some archaeologists, as shown by the following statement by Betty J. Meggers (1955: 129):

The strides that have been made in recent years indicate that far from being a handicap, there is a considerable advantage in being forced to deal with culture artificially separated from human beings. Shorn of the complicating and confusing psychological reactions of numbers of unique human personalities, cultural processes emerge in a stark and clear light. The remarkable accomplishment lies not with the archaeologists who have recognized and profited by this advantage, but with those ethnologists like White (1949) who have been able to penetrate to fundamental cultural insights through the psychological maze.

Archaeologists, forced by the nature of their materials to study culture shorn of its living human element, can accept a superorganic concept of culture more easily than ethnologists. I agree with Bidney (1953: 32) that archaeologists are committing the metaphysical fallacy of misplaced concreteness when they attribute power of activity to cultural ideas: There can be no culture without the individuals and societies which are the efficient causes of the cultural processes.

Evolution and Cultural Laws

New Interpretations of Aboriginal American Culture History, published by the Anthropological Society of Washington, contains a number of articles on archaeological theories. Of special interest for this discussion is the article by Meggers, who says (1955: 118):

If we are to accept any or all of these 'new interpretations of American culture history' we must be prepared to accept the assumptions or principles upon which they are based. This includes some form of evolutionary development of culture, some form of environmental determinism, and the recognition that detailed cultural resemblances are evidence of cultural diffusion whatever the obstacles to its occurrence appear to have been. All of these are old ideas in anthropology, all have been enthusiastically espoused and vehemently denied. Our problem is to decide whether they are scientific theories that can be accepted as working tools, or whether they are products of distortion and ignorance of the facts, and, therefore, misleading if not useless.

Citing the achievements of physics as a prime example of the image of "Science" (with a capital S), Meggers brings out the point that many laws of physics are not descriptions of processes that are followed invariably and uniformly by every atom, but instead are statistical averages. The law of mechan-

ics, for instance, states that molecular motion increases with the rise of temperature, and that this increase proceeds at an even rate. Since the heavier particles always move more slowly than the lighter ones, the law cannot apply to each individual particle in a mixture, because at any given moment a particle may be traveling at a great velocity or may be almost motionless. Since the majority would be moving at a rate close to the average, the proposition is statistically true. Visualizing the deviant particles as cultures which, by application of a statistical approach, can be brought back into the fold of a general law, Meggers (1955: 120) says:

If we think back a moment to the physicists' statistical law of mechanics, it will be noted that the kind of association anthropologists make between environment and culture is similar to that made by the physicists between temperature and molecular motion. Their law says, when the temperature rises, the molecules move faster *on the average*. Our law says, as the environment improves in subsistence potential, the culture advances in complexity, *on the average*.—Since such statistical laws work so well for physicists, there is no reason why anthropologists should not give them a try. There is everything to gain, since such an approach not only permits prediction but also suggests new avenues for investigation.

Meggers discusses two other types of physical laws and their application to cultural phenomena. According to the first law of motion, the natural motion of a body occurs at a uniform speed in a straight line. Actually, this is not observed anywhere in the universe, since disturbing factors, such as gravitation, distort the motion. The law merely describes what would happen if there were no disturbing forces; and the fact that there *are* disturbing forces explains why it does not happen. Meggers considers the theory of cultural evolution a law of this type: Few cultures passed through the postulated stages of development because diffusion was a distorting factor; but this need not destroy cultural evolution as a law. The third type of physical law is purely descriptive, and consists of statements of facts describing an observable situation to which there are no exceptions. According to Meggers the assumption that a complex composed of a number of distinctive elements or traits can originate only once, and that all of its occurrences must be related, can be equated with Kepler's physical laws of planetary motion. Meggers apparently believes that diffusion studies have provided evidence sufficient to elevate the above assumption to the status of a descriptive law.

Meggers' main theme is that there should be laws of culture that have as

much validity as those found in the physical world. Through the discovery of such laws, anthropology will achieve a respectability comparable to that of physics and other sciences. The physicists' admission "that their laws are not the hard and fast, tried and true, tested and proved, permanent and immutable formulations that we have conceived them to be" (Meggers 1955: 123) has great significance for anthropology, because it permits the acceptance of cultural laws that have been rejected for lack of proof or because, exceptions were noted. But, in my opinion, the quest for laws and the application of scientific method do not necessarily make anthropology a "science." Meggers does not seem to distinguish clearly among postulates, axioms, hypotheses, and laws; for her, laws and generalizations are identical. Bidney (1953: 277) had previously pointed out a similar confusion in the cultural laws put forth by Herskovits (1948).

Cultural Evolution

The concepts of cultural evolution, environmental determinism, and cultural diffusion are as important for modern anthropological research as they were fifty years ago. It is imperative that we recognize the varying philosophical interpretations behind these concepts if we are to make intelligent use of them. In recent work of major significance, Bidney (1953) focused critical attention on the theoretical foundations of anthropology. However, little cognizance has been taken of Bidney's findings, in spite of the archaeologists' claim that they are interested in theory and are attempting to use the theoretical concepts of anthropology to explain archaeological phenomena.

The concept of evolution is very much a part of anthropological thought but it does not have the same meaning to everyone. Bidney (1953: 236-49) showed that Rousseau and other 17th and 18th century philosophers postulated the evolution of man, through a process of self-perfection, from a prehistoric, precultural state of nature to a cultural state characterized by such basic cultural traits as language, the family, and morality. Thus, the idea of cultural evolution did not begin with the advent of the Darwinian theory of biological evolution in the 19th century, although interest in cultural evolution greatly increased at that time, and new ideas were introduced. Workers, such as Tylor and Morgan, became interested in the natural history of cultural development and sought to evaluate the stages of cultural process. Cultural evolution was conceived as developing from the simple to the complex according to definite psychological

laws, and all cultures were assumed to pass through independent, parallel, universal stages of cultural progress. The three main stages, as conceived by Tylor, were savagery, barbarism, and civilization. These three stages in the process of culture history were not "laws" to Tylor, but were empirical generalizations by which the continuity of culture could be comprehended. All peoples did not necessarily pass through the three stages to attain the same degree of civilization. The basic theme of Tylor's evolutionary theory was a continuity of culture history involving development from a lower to a higher degree of culture. Many of Tylor's followers, however, identified extant native peoples with the three stages of culture history for which empirical evidence was lacking.

As Bidney (1953: 247) pointed out, it was against these pseudohistorical reconstructions that Boas and the diffusionists reacted. While opposed to the thesis that culture always developed from the simple to the complex, and that there were definite stages of cultural evolution, Boas was not opposed to the concept of evolution within the context of a given culture, or to the idea of progress within certain aspects of culture. Boas and many of his students concentrated their attention upon particular cultures and the diffusion of culture traits over limited areas. Consequently American anthropological studies emphasized particular cultures, and the culture history of mankind as a whole was pushed into the background. The evolutionary schemes of the 19th century became unfashionable in the light of archaeological and ethnographic research conducted by Boas and others early in the 20th century.

Interest in cultural evolution was recently renewed. Leslie White (1949: 338-39) and V. Gordon Childe (1951: 32) attempted to keep the evolutionary concept of cultural stages alive by relating these stages to the culture of mankind as a whole. They excluded, as irrelevant, distinctive cultural traditions and local variations that developed as special adaptations to certain environments or as the result of special historical trends. But Steward (1953: 317) showed that this approach, which he terms "universal evolution," can give only postulated cultural sequences that are so general as to be of little use. Steward considers the vague postulations of "universal evolution" even less valuable than the old "linear evolution" of Tylor and Morgan, which did attempt to explain the association of specific features of culture or why

certain cultural institutions followed a definite development in time. Steward's solution to the problem lies in his concept of "multilinear evolution," which has as its basis an interest in determining recurrent forms, processes, and functions, rather than broad schemes and universal laws. The explicit interest of this view is in parallelism and causality as viewed from the perspective of particular cultures. Cultural similarities may involve outstanding features of whole cultures, or only such special features as clans, kinds of social classes, or other particular items. Multilinear evolution deals with parallels or similarities distinguished by their limited occurrence and specificity. For this reason, Steward believes, the major methodological problem of multilinear evolution is that of finding an appropriate taxonomy of cultural phenomena. A taxonomy suggesting significant parallels will become possible if interest is centered upon functional interrelationship of cultural features and the processes by which cultures are adapted to environments (Steward 1953: 323).

When Meggers suggested that a law of evolution can be formulated with the same exactitude as those of the physical sciences, she made no distinction among the various concepts of evolution espoused by Steward, Childe, and others, which are so diverse that it seems inadvisable to give the broad, undefined concept of evolution the status of a law. I agree that cultural evolution is very much a part of anthropological thought; but for it to be a useful concept, it must be clearly defined.

Bidney's (1953: 46-47) theory of "emergent evolution" synthesizes the principles of levels, continuity, and plenitude by limiting the role of each in relation to the others. While adhering to the principle of continuity, this theory allows for disparate forms of being. The postulate of actual empirical levels, with the necessary intrinsic relations of higher and lower phenomena, is accepted. The relation of higher and lower levels of phenomena is one of polarity, so that while each level is dependent upon another, it also enjoys a measure of autonomy. New forms of being supervene upon the material provided by the lower levels. There is continuity in the development of natural forms combined with the emergence of "novelties," or "qualitative variations." There is a "hierarchical continuum" of limited possibilities evolving in time. There is neither absolute continuity nor absolute discontinuity. According to Bidney (1953: 47):

Evolution is not a completely intelligible process, for we do not explain by reference

to it why and how it is possible for new variations to emerge; all we can say is that they are 'spontaneous' and 'chance variations,' which is but another way of saying that we do not know how they originate.

The entire course of nature is not explained merely by tracing the known conditions and interrelations of the various kinds of phenomena. Bidney warns that an empirical approach requires that we seek to understand all the complexity and interrelatedness of phenomena. None of the data should be explained away in order to obtain a simple coherent theory. The penalty for ignoring such data as the relations between levels of phenomena, is that we become victims of our abstractions, and the dynamic aspects of nature we hope to explain are denied. I believe archaeologists can find Bidney's approach to cultural evolution useful. It is not restrictive and it appears to indicate clearly the "facts" of cultural evolution as now known. It recognizes the existence of levels of culture that can be methodologically classified. For practical taxonomic purposes, the various levels can be studied "as if" they were independent of lower or higher levels, but it is also imperative that we remember the interrelationships between levels. No level is completely intelligible apart from the levels below it, but a higher level cannot be reduced to a lower merely because it shares common traits with the latter.

Until North American archaeologists become fully aware of the theories that have guided them in the past and still—often silently—steer them today, we shall not achieve the kind of progress Sears calls for in this article. As archaeologists, we can not throw light upon the social and religious systems of prehistoric cultures until we admit that people, individually and as groups, were more important than potsherds, sticks, and stones. The archaeologist must certainly use every technique at his disposal to gain the fullest knowledge of past ways of life, but we must never again let these techniques become the end product. We can no longer wait for the "bit by bit" discovery of a pre-existing order in the culture-historical universe, but must diligently seek out the interrelationships of cultural phenomena if we are to discern the hows and whys of cultural process. If the theories of the past have been inadequate, let us turn quickly to new concepts and good hard work, for time is running out. In many areas of the world it is already too late.

By PAUL R. DUCEY*

My reaction to Sears' article is somewhat equivocal. This is not due to any

inadequacy in his presentation or to any lack of clarity in his interpretation of archaeological reports. The difficulty lies with the criteria he employs to evaluate the accomplishments of scholars who are judged not by their real effectiveness but rather by their publications.

American archaeologists, especially when their interests are concentrated on North American archaeology, hesitate to publish their views on "processual interpretation." In this respect they deserve the criticism most tersely presented by Taylor (1948) and further documented by Sears. Heizer (1959) has implicitly confirmed this position; his source book in the method, theory, and techniques of interpretation of archaeology presents a balance of selections from three main areas: North America, Europe, and the Near East. It is significant that the majority of the readings dealing with method are drawn from North America. This area provides no illustrations of the reconstruction of specific events or the reconstruction of life and customs, and only one selection relating to the problem of prehistoric ecology. Many of the selections dealing with cultural reconstruction and the problems of ecology are by U. S. archaeologists, but they refer to other parts of the world.

Countless reports, many of which literally breathe life into the artifacts and features left by long-forgotten groups of men, attest to the ability of North American archaeologists to reconstruct the cultural development of other prehistoric peoples. But when attention is turned to North America, the picture changes. Archaeologists who have described the cultures of other countries in vivid terms refuse to give their material the benefit of their full descriptive ability. The evidence is discussed as a sterile collection of traits, types, and series.

Actually, this criticism is not fully justified. The published reports may be tedious accounts of method and cataloguing of specimens; but in the classroom this material receives thoroughgoing interpretation based upon analogy or reasonable inference. Imagination, based upon experience and the factual evidence, also plays a part in these reconstructions. Students accept the reconstruction in the spirit in which it is offered. Should the same reconstruction be offered for publication, however, the author would apparently face the loss of professional standing. It is, of course, permissible to write such articles for certain non-professional magazines, with the tacit understanding that what is said is intended for a popular audience; and frequently the authors feel obliged to apologize to

both students and colleagues for a transgression tantamount to scholarly prostitution. By mutual agreement such articles pass into a nether world.

Perhaps more than anything else, a greater willingness to present full-blown reconstructions in recognized journals is needed. The interchange of valid and constructive criticism—not condemning retorts—should lead to a more complete appreciation of prehistoric life, times, and development. More is involved here than the question of anthropological theory. If archaeologists, in the tradition of anthropology, do not interpret their findings for the public, others will. This alternative is hardly desirable; compare the excellent and stimulating works of Childe, Geoffrey Bibby (1956), or Carleton Coon (1957) with the popularizations by Roland Robbins (1959) or Harry Stafford (1959).

Archaeologists have an obligation to serve the public. Their material should, first of all, be understandable and meaningful to fellow archaeologists; but unless the major findings reach the public, the effort is wasted. Personally, I do not want my students or other interested people to say, "My, isn't that interesting"; I want them to realize that prehistoric man faced and solved problems common to all mankind, and that remote events can contribute to an understanding and appreciation of our own society and culture.

For what it may be worth in stimulating further discussion of the objectives and levels of processual interpretation, I offer here the approach I present to my students. I do not claim or imply any originality or priority for this scheme, but I cannot cite any specific sources. It has grown out of my research and the interplay of teaching undergraduate students. I can present my views most succinctly in outline form.

I. Anthropological Approach

A. Field Objectives

1. Systematic recovery and identification of prehistoric evidence (artifacts, shelters, burials, etc.).
2. Preservation: Complete documentation of the evidence recovered.
3. Restoration: Restoring anything from an artifact to a community.

B. Primary Objectives of Interpretation

1. Chronicle: Charting the connections in time and space of the types of material, stressing the origins and diffusion of cultural traits.
2. Historical: Tracing the continuity and change in commu-

nities, cultures, and culture areas.

3. Reconstruction: Re-creating the way of life of the people, with special attention to techniques of manufacture, subsistence, economics, social and political structure, religious patterns and spiritual beliefs.
4. Functional: The significance of the material remains as they contributed to the culture, but emphasizing their contribution to fulfilling the needs of the people.
5. Developmental: Seeking regularities either as recurrent events or world-wide stages of development; in either case, a search for the general principles of man's achievement.
6. Explanation: In so far as possible an appraisal of the significance of the culture in understanding contemporary peoples.

II. Historical Approach (Including parahistory and classical)

A. Field Objectives

1. Systematic recovery and interpretation of historically *known* material.
2. Preservation of structures and artifacts.
3. Restoration of national monuments.

B. Primary Objectives of Interpretation

1. The six objectives of anthropology; but, typically, leaving the full interpretation to historians and philosophers, stressing the heritage of major extant civilizations.
2. Reconstruction of the fame and fortune of nations and their leaders.
3. Aesthetic appreciation.

I have only one real point of difference with Sears: I cannot understand his view that cultures are fluctuant while social groups are clear cut. I do not wish to quarrel over terminology; but social groups are unstable, fluctuating aggregates of persons, who are ordered and regulated by their culture. The culture, while changeable and changing, is far more clear-cut than any form of society.

Fundamentally, I am in complete agreement with Sears. If I understand his position, we need a more thoroughgoing, outspoken approach than has been attempted by North American specialists in print. His suggestions are valid and, what is more important, challenging. At the same time, I feel a very real obligation to carry the level of in-

terpretation one step further than its meaningfulness to anthropological theory.

By HAL EBERHART☆

There is much to applaud in Sears' presentation, and little with which I would disagree. During the last twelve years, a number of authors have exhorted U. S. archaeologists to move beyond their typologies, classifications, and chronologies into the realm of reconstruction, interpretation, and explanation. That all archaeologists should carry their active interests to the level of science—in the sense of a search for timeless, spaceless regularities—seems too much to ask; and Sears is not demanding this, although (as I understand him) he conceives it as one of the ultimate objectives of archaeology as a discipline. What he is asking is that archaeologists bring their data up through several levels (or logical steps) of interpretation to the point where they are comparable to and compatible with those presented by other fields, and can be used by other archaeologists and cultural anthropologists who are inclined toward the pursuit of regularities. This request seems eminently reasonable.

One reason archaeologists have been slow to enter the area of culture and cultural reconstruction may be that it seems a long leap from the construction of artifact classes to the postulation of social classes. It is, therefore, highly important to point out what has been done as a guide to what may be done. Because excavation is destructive, and because awareness of problem does affect observations in the field, the reconstruction and interpretation of social and religious structures should be part of the objective of any excavation. It may be unduly pessimistic, however, to believe that meticulously reported excavations made without such a goal in mind can never yield appreciable data of this nature when re-examined by others: for, hopefully, new techniques for reconstruction will be developed, and old ones sharpened, so that previously published material can be utilized. Nevertheless, the excavator of a site is the person best qualified to milk his data of all the information they contain.

Sears seems to imply that until the time and space dimensions are known at least in skeletal outline, movement into the next level of investigation must be delayed. I do not see that the two types of problem are incompatible, or that their pursuit cannot proceed simultaneously, if only on a limited spatial scale. Archaeologists apparently have a

very strong tendency to wait until all the facts are in, in spite of the certain knowledge that they never will be.

I would also emphasize this point: The probable simplicity of social and religious systems among prehistoric hunters and gatherers in a poor environment should not relieve the archaeologist of making whatever inferences he can from the excavated remains of these cultures. Even if such peoples had relatively uncomplicated systems, this fact is as important a bit of knowledge as that the agriculturalists of later periods had complex systems. It follows, therefore, that the details of the simpler systems, while perhaps harder to recover, are as significant as those of the systems of later cultures.

By CHARLES H. FAIRBANKS☆

Sears' suggestions seem entirely praiseworthy. Archeologists do need more problem-directed excavations if we are to reconstruct the cultural processes that have produced specific archeological complexes. However, I wonder whether this sort of archeology is really possible at the present time. In the southeastern United States, the only problem-oriented archeology with which I am acquainted is that concerned with specific problems of restoration of noteworthy sites. Sears, himself, is engaged in a massive survey of the Gulf Coastal Plain, but this is not, in its nature, capable of solving the specific problems which must be attacked first. Most of us, without any coherent plan, find ourselves rushing from one emergency excavation to another. Not all of southeastern archeology is salvage work—although much of it is specifically that—but we are all engaged in salvaging sites before the destructive approach of urbanization or tourism.

This is admittedly a sorry situation. Most archeology in the United States is conducted by colleges and universities where the main activity is teaching, and research must be tailored to fit class schedules. Museums, although free from this academic burden, are often forced into emergency programs that have little relation to intelligently planned problem-approaches. Archeologists do need the specific search for specific answers, but I wonder whether many of us will have the opportunity of seeking them. As Sears points out, they will not be found unless we plan to look for them. And he is surely right that the standard test-pit or test-trench approach will seldom provide more than refined chronologies.

For me Sears' criticism resolves into the question of what sorts of problems archeologists should set up for solution.

It seems to me that problems related to the ecological relationships are more basic than those dealing with such derivative aspects as religious and social forms. Sears rightly points out that we should aim at the reconstructions of cultures. This should include sufficient reconstruction of the technology so that we can reconstruct the nature and intensity of the archeological culture's exploitation of its environmental resources. Until we know this basic technological aspect of the culture, we shall not be able to reconstruct its religious organization or social structure with much validity.

As a case in point, Sears (1954, 1956) has given us a very valuable and highly useful reconstruction of the burial ritual at the Kolomoki Mound site in southwestern Georgia. We do not yet have any clear idea of the subsistence technology of the Weeden Island group involved, and cannot get this knowledge without extensive and intensive (and problem-oriented) excavation of the residence areas. From the extensive construction of burial mounds and the possible construction of temple mounds, we would guess that this group had an agricultural technology. The relative amounts of cultivated and wild foods in the daily diet, the specific type of farming practices, the hunting techniques, the composition of work groups, and many other similar questions must be answered before our discussion of the religious and social organizations can be much more than interesting speculations. Sears says that archeology can "usually provide clear, sharp information about population size." I seem to have missed the boat here; I have the impression that archeologists still find it very difficult to state precisely the relationship of an aboriginal population to the carrying capacity of its territory. Certainly we shall need to know this carrying power, the human density per acre, and the temporal duration of the site's occupation before we are ready to understand the cultural significance of our evidence for ritual and social classes.

In the Southeast, the Creek pattern of centralized "mother" towns with a ceremonial square and outlying farmsteads seems to extend as far back in time as the Mississippian phases of the prehistoric period. These temple-mound towns had a distinctly metropolitan character, with more varied arts and crafts, and probably more complex social classes, than the small agricultural homesteads scattered across the countryside. Cultural reconstruction built solely on the burial mounds of the metropolitan towns ignore important components of the total culture. In Alachua County, Florida, Goggin has

identified more than 300 aboriginal sites; in Leon County we know of about fifty sites. In neither case are we ready to map population density for any specific period; and until some such ecological study can be made, any reconstruction of the culture is rather shaky.

Sears moves from discussing the reconstruction of ritual to speculating about the role that stylistic analysis can play in determining the kind and extent of craft specialization. He suggests that examination of Weeden Island pots indicates the existence of specialized, presumably full-time, potters. It seems to me that in this area we run the danger of again falling into the pit of blind artistic criticism. Are specialized ritual objects always the work of specialized craftsmen? Do not many or most Pueblo men turn out elaborate and specialized ritual objects between stints in their wives' corn-patches? Again, extensive and intensive investigation of village areas to discover possible workshop sites of a specialized nature would put this sort of speculation on a much firmer basis.

Incising of shell; marble statues at Etowah; extensive copper work; mass-produced specialized ceramics: all these may represent specialized production. It is the circumstances in which they were produced, however, and not the style or slickness of surface finish, that determines the degree of specialization. Here again, I should like to add to Sears' suggestions. My thought is that we need more basic information about the technology and subsistence level before we can make valid reconstructions of ritual and social stratification. In the realm of ethnology, we generally assume that the specific type of technology determines such traits as residence pattern and work-group composition. On this foundation are built such phenomena as clan structure, social strata, and religious complexes. It seems to be putting derivative features first if one attempts to deduce these forms without a valid preliminary reconstruction of the technological base.

The foregoing paragraphs are not altogether an attempt to refute Sears' basic position. I thoroughly agree that we need more problem-oriented archeological excavations, more extensive unit-digs, and fewer strata cuts. It is possible, and profitable, to reconstruct whole cultures or selected aspects of a culture. This can probably be done only by properly conceived excavations. I think archeologists should direct more attention to extensive and intensive excavation of residence units.

It is probably no accident that in the United States the most successful reconstructions of cultural units have been made in the Southwest, where much of

the archeology has concerned residence units.

By D. H. GORDON☆

Sears' review article is in general concerned with the ways and means by which an archaeologist can discover what really happened in prehistoric times. The neglect of everyday articles of utility, of pots and household utensils, by archaeologists of earlier generations, who were hunting for objects of spectacular form with which to enrich their museums, caused a revulsion in archaeological direction which, as Sears remarks, "through manipulation of increasingly refined techniques of artifact style analysis has become an end in itself."

Many years ago Sollas, with the logical idea of working from the known to the unknown, attempted to arrive at the knowledge of what really happened in prehistoric times by taking as examples peoples who had survived into recent times in what—judging by their artifacts, their culture as food gatherers, and their environment—seemed to him to be a state of existence parallel to that of the ancient hunters. In spite of the fact that no one has produced a more plausible picture of what really happened in those prehistoric periods with which Sollas was dealing, his thesis was set aside and not replaced by anything that did not say the same thing in longer, less comprehensible, words. This approach to the problem is clearly recognized by Sears, where he generally approves Deuel's reconstructions of the social and religious organization of the Hopewell culture based on that of the 18th century Natchez Indians, and also in his penultimate paragraph where he says

These will . . . give us a greater stock of comparative materials, such as the identification of historic peripheral survivals for reconstruction of prehistoric social and ceremonial systems.

It can be freely acknowledged that questions concerning the interconnection of food-production, population size, and urbanization are of the greatest importance with regard to any overall assessment of cultural development. What ecological conditions would produce herdsmen or cultivators; did these means of production develop separately, and did they have several areas of origin or one such area from which the idea was diffused; did dairy farming and agriculture exist separately side-by-side in one community, and if so, who started mixed farming and when and where? We are given to understand that if information on such points is sought with sufficient diligence and care, we shall find it. But it is hard enough at

times to distinguish a cultural assemblage, let alone a social pattern—that detailed picture of how a pre-literate people really lived. For example, Sears points out that the Swift Creek “culture” is actually a ceramic tradition. In the Indo-Iranian borderland we have a number of quasi-cultures, many of which are only distinguished by ceramic types, about the origins and changes of which we have little real knowledge. Where we cannot isolate a community in a certain stage of its development, how can we achieve a picture of it as a social, economic, and religious unit? Where there is a written record we can apprehend something of the thoughts and acts of men of past ages, but without this record much of our reconstruction must be pure guess-work, though no doubt many will hotly contest this view.

Warfare, which is mentioned in passing in connection with technological advancement, is a matter of great social importance. The origin of warfare is a subject which has produced much speculative and tendentious argument, particularly from those seeking to establish that fighting is not an occupation natural to man. Be this as it may, the causes of war are several and include such things as economic pressures and prestige, and in time war between traditionally antagonistic groups may become almost ritual. Economic pressure and economic advantage have always been prime factors in causing war, but though the whole matter of war and its causes is most complex, it is a social phenomenon of the very greatest importance, which either as a threat or as an event has conditioned the living of almost any community at any period one may choose to name.

Bearing in mind the objects of archaeological excavation, surface collecting and strata cutting have their clearly defined uses. The former is part of the necessary reconnaissance; the latter, an indispensable preliminary enabling the excavator to comprehend the potential of the site and apprehend the problems that are likely to arise. Only after an accurate survey and a preliminary assessment of the site have been made will it be possible to effect that total layer-by-layer removal which will give us an over-all cultural picture of each successive settlement. It must be a very simple site which can be assessed as a local social group instead of in terms of cultures and phases. Cultures are fluctuant, but so are local social groups. It is true that any aggregation of families inhabiting a settlement constitutes a local social group, but they may be invaders who totally displaced their predecessors, or they may be a new group, settling on a site which, though previously occu-

ried, had been deserted for hundreds of years. These three peoples could all have been successively present on the same site and can display fundamentally different cultures. One such group might continue in occupation long enough to evidence different phases, probably as a result of outside influences and contacts. Provided the documentation is good, the parallels unforced, and the limitations of each discipline clearly recognized, it is difficult to see what method having real meaning is available for the reconstruction of the social habits of prehistoric peoples other than an intelligent co-operation between archaeology and anthropology.

By WOLFGANG HABERLAND*

This article by Sears constitutes a major contribution to the revaluation of North American archaeology (and certainly to American archaeology in general, too). Obviously, this is a time of new approaches and of a search for new bases and theoretical structures that will give this science a firmer ground to build upon and new goals to be gained. It should not be surprising, therefore, if various commentators express certain dissents.

Agreeing in general with the statements made by Sears, I should nevertheless like to remark on some points about which I hold slightly different ideas. If this further stimulates discussion, the intention of this paper is fulfilled.

Sears says that he does not intend to criticize description and analysis of artifacts, and states that such studies are basic to archaeological work. Nevertheless, in the same paragraph he infers that this kind of study has “become an end in itself.” I doubt that this is true of any serious archaeologist, in the U.S. as well as in any other part of the world. Such work is, and will always be considered, a preliminary; but one which no archaeologist can omit. It is the foundation for any reconstruction of events or aspects, whether historical, religious, ecological, or sociological. Nobody denies that every one of the above-mentioned aspects is important for the re-establishment of the original culture.

However, the question that generates this discussion is in the last analysis concerned with something deeper and more remote from daily studies and field work: the ultimate goal of archaeology itself. As I see it (and I think this is not only a problem of North American archaeology, but one concerning the whole of world archaeology), there is not one goal but at least two of them, reflecting the multiple connections of archaeology (and of anthropology in

general) with other sciences, and its historical development from them. They may be called the *historical goal*, and the *goal of comparison*.

In the case of the historical goal, archaeology is a part of human history—cultural, political, or otherwise. It attempts to reconstruct events back from the point where written history ends. Here distribution in time and space is of prime importance, since no culture, however well its aspects are known, will have any value in this context if these two dimensions are not firmly established. It is in this sense that all “archaeology” concerned with areas and times that lacked written records is essentially “prehistory.”

The winning of the other goal does not place so much emphasis on time and space, although these play a role in certain problems. Here the main object is the comparison of cultures to gain certain—possibly universal—“laws,” or at least insights about the universal functioning of culture and its interplay with the environment, climate and many other factors. In this case, time (and therefore studies aimed at fixing this dimension) can *sometimes* be omitted; but this can never be done if one has the first goal in mind.

Most archaeologists will not consciously aim at either end, especially when engaged in field work; but unconscious inclinations toward historical or comparative problems can and will colour their reports and the selection of data presented therein. The factor involved here is a human one, which can hardly be suppressed. Teamwork by persons with different inclinations, different interests, and different problems in mind would be ideal for the evaluation of data, but this is rare, and probably will always be infrequent. Nevertheless, a thoroughly-trained field archaeologist will not consciously overlook any factors during the excavation—or at least, only those that are unrecognized now but may be available at some future time through new techniques.

If we reflect upon the final and definite destruction of the sites through excavation, and hesitate because we might overlook some slight clues to special problems that have not yet been conceived, then ultimately we shall have to stop doing any field work because fifty years from now there will certainly be techniques not yet dreamed of, which will, I hope, enable us to regain more from a site than is now possible even with the most refined methods. The science of archaeology is progressing, not only in knowledge but also in technique, as one can see in the development of field work and reports

since the beginning of this century. But the development of techniques is only possible through experience, as in any other science; and ceasing to excavate solely because we hope that in the future more knowledge can be obtained from a site would make an end to all progress.

This should not be viewed as a passport to sloppy field work or to prolonged delay in publishing field reports (which I consider a major sin for field archaeologists: unfortunately it is a frequent one and sometimes not their own fault).

A thorough field report contains many aspects which, not yet meaningful, will allow reconstruction of other facts in later times, in the light of new problems and new experiences. Clues that are now only recorded may later acquire significance through advanced knowledge. The essential thing, therefore, is not so much a problem-oriented excavation, as Sears proposes, which may destroy clues to other problems not yet conceived, but the recording of *all* features, regardless of their present value.

Sears also regrets that only a few descriptions of houses, burials, and sites are to be found. This is certainly due in part to circumstances beyond the control of the archaeologists—e.g. soil and climate conditions. Favorable conditions, such as those in the Southwest U. S. and along the Peruvian coast, are relatively rare; but they have always, through their wealth of material, stimulated research beyond the limits of material culture. On the other hand, it is true that test-digging will very rarely result in such descriptions, which can be gained only by extensive excavations. Nevertheless, test-holes are extremely necessary, not only for the historical approach but also for the kind of study proposed by Sears. If the excavation of a whole site reveals a set of patterns for a certain culture, it is of prime importance to verify these through at least one control, since it is quite possible that these patterns are the exception to the norm. Without knowing which of the other sites belong to the same culture, the chances that the next excavation will run into the same culture are sometimes very slight. Systematic testing of all sites, as is often done in Old World archaeology, not only provides indications for the distribution throughout the different time levels, but also makes data available for the selection of sites to solve special problems.

I hope that these comments will not be interpreted as a criticism, but as an attempt to explain why main emphasis is often attached to the time and space dimensions.

Another unfavourable point is that

the operations envisaged by Sears are mostly beyond the time and the financial resources of the individual archaeologist. They require a co-operative effort, which is often difficult to obtain. Nevertheless, there is one possibility for a further approach to cultural implications, which in my opinion is not stressed enough by Sears, but is well within the means of the individual student: this is the individual human being. He is the essential factor in a cultural entity; rarely an innovator, but always the bearer of its contents. He is the basic unit of any religious, social, political, or economic entity; but most archaeologists curiously tend to ignore him, concentrating on the more complex unit instead. The best approach to the individual may be the evaluation of burials, which are often treated only as deposits of objects. What is often overlooked is the fact that all these objects had a meaning to the dead person and/or to the culture he represented. They should reflect such basic elements of daily life as tastes, beliefs, occupation, social standing, etc. These aspects are rarely treated in the reports. Even sex is often determined only by means of Physical Anthropology, and not by the association with certain patterns of grave-goods, which should reflect this difference most clearly. I believe that through this approach—building up the patterns of culture through the individual—many of the problems indicated by Sears can be solved, while at the same time it would not require efforts which are beyond the material possibilities of archaeology.

By FREDERICA DE LAGUNA☆

Sears has given us an interesting and provocative article. His characterization of a mound as a "fossilized ceremony" is felicitous, since this means that it should be understood as evidence for a particular set of particular events carried out by a group of people. In fact, he could have generalized still further, insisting that every archaeological site is a fossilized set of events. These were particular events. I would not call them "unique," because, while they happened once at this particular place, they were conditioned by, or expressive of, the cultural background, which presumably fostered similar activities at other times or at other places. It is obvious that the archaeologist who wishes to reconstruct the past must be aware of this dual character of the remains as at once individual or particular and also as characteristic or expressive of a whole culture. Resemblances shared with the "fossilized" activities of other sites will help the archaeologist to fill in the gaps in his own data and make his

reconstruction with greater sureness.

Sears urges that the interpretation be "tightly related to physical evidence," but there is, of course, no short-cut method or sure formula which can be applied to the data. Essential are a broad ethnographic and archaeological background, an inquisitive sense of problem, and above all, a creative imagination linked to a respect for the hard fossil bones both as stubborn facts and as the relics of once-living fellow men. The scientist must be ready from time to time to lay down his trowel and look about him with a fresh sense of wonder, to ask himself what this evidence once meant in terms of human activity and living.

In this article, Sears limits his discussion to what can or should be attempted in reconstructing social and religious systems, and only mentions the necessity of basing such studies on an understanding of ecology, technology, and economics. He might well insist, however, that all these attempts at reconstruction be pursued together, as parts of a single problem, since what is to be understood is a cultural whole, a unified way of life which was not divided into such categories for those who lived it and who left these remains. Old-fashioned ethnography that was concerned with total culture, rather than new-fashioned social theory that deals too exclusively with social and religious systems, will be, I think, the more useful partner for archaeology. I do not believe that the archaeologist must set for himself as a final goal the formulation of regularities ("laws") in cultural processes. He has perhaps a greater contribution to make in the reconstruction of particular cultural histories; a unique contribution concerning sets of particular events.

By WILLIAM J. MAYER-OAKES☆

To comment briefly and usefully on Sears' paper is difficult because of its comprehensiveness. But even though it is wide-ranging, this paper strikes at what I feel to be the most important segment of the leadership of current archaeological research. My comments shall first indicate what I think is the significance of the paper; next I shall raise some questions, then a few criticisms, and finally, I shall present some additions to the topic.

Significance

Sears' paper as a whole can be described as a programmatic statement which points out necessary and fruitful directions for archaeological research. The substance of this program is strongly conditioned by the fact that Sears himself has done, and is publish-

ing, the kind of archeology he wants others to do and publish. Anyone not familiar with his previously published work is bound to be impressed with the skill and energy Sears has already poured into his studies of southeastern U.S. religious and political systems. What is more, his results have clearly justified the effort, and augur well for the opening of new vistas to the "dirt archeologist." This article is not a work of armchair or library research, but ably represents the current attitudes and "philosophy" or theory of an active anthropologist who works with dead cultures.

Basically, Sears argues that field archeologists should change their attitude. He points out the need for a level of competence in the area of interpretation that is at least comparable to the achievements to date in the field of chronicle. This focus on the need for more adequate interpretation with a broader scope is the crucial point. Sears points out how archeological problem-orientation effectively conditions the nature of the expectable results. Because of his own accomplishments in this area, I think Sears' words will be heeded by many North American archeologists. He tempers his enthusiasm for "social archeology" by reminding the reader of the ever-present need to derive valid interpretations from empirical evidence and relate these interpretations to this evidence.

Questions

I have two main questions to ask Sears on this topic. First, do we really know enough basic time-space descriptive archeology to abandon a broad "salvage" orientation or an all-inclusive field orientation for the more specialized social approach that Sears recommends? This is the old problem of the ethnographer: Does he go into the field as a blank book or a sponge simply to soak up whatever he can? Or does he go in to gather specific kinds of data for application to predetermined sets of problems? I feel that a solution to this question is no nearer now than it ever has been. A middle position is what most often develops, and so I feel that a specific archeological field approach must be qualified by this realization. The last five years, for example, have seen a tremendous change in the generally-accepted interpretation of the relationship between Archaic and Paleo-Indian cultures in the eastern United States as well as in the detailed facts available for interpretation. This specific example, as well as the situation in Nuclear America, causes me to wonder how much emphasis on the collection of basic all-inclusive field data is still needed. Because of the destructive

nature of archeological excavation, I feel that the ethnographic parallel is inadequate as a basis for action.

My second question develops out of the first: Assuming that an archeologist desires to work in the way Sears suggests, but also wants to work in a geographic area where the basic framework of descriptive data is obviously incomplete, how does he proceed? I think the field realities here, particularly in the study of the complex societies of Nuclear America imply the need for a much broader attack than has been possible in the past. A solution may be teamwork of a sort that has been seldom tried. Interdisciplinary specialists working together can provide valuable breadth for the questions raised about past human societies, but intra-archeological variety is also needed. How about a team of three or four archeologists, combining basic interests in field technique, problem-definition (both chronological and historical), and skill in interpretation?

Criticism

A subsidiary question has to do with the mechanisms for interpretation. I see this as a most specific need, about which Sears says little. Thus, I would criticize his paper on this score. While one can agree that more and better interpretations are needed, the question of how to get these arises immediately. Describing the procedures of, field-recording as various kinds of compromises. Sears tells us little about the differences between recording to reconstruct a funeral ceremony and general recording. For perspective on this view, reading about his research at Kolomoki is most instructive; but the whole area of techniques for gathering data for various purposes needs to be examined in detail.

Another critical comment I direct at the statement that "evidence useful for social and religious reconstruction and interpretation cannot appreciably be derived from an ordinary survey . . . nor . . . from strata cuts or test pits . . ." Here, I think Sears underestimates the multiple-purpose value of much archeological field work. So much depends upon the attitude of the fieldworkers (as Sears well knows) that I suggest we need, in some areas, to collect basic field data with several kinds of problems in mind.

My own experience with a strata-cut approach in the Valley of Mexico suggests that the complex archeological societies (those that are or develop into civilizations) need much basic time-space descriptive work. If this is done with the attitude that this information

is immediately going to be specifically useful for more accurate interpretations of the past, then the work of describing and relating ceramic types is not an end in itself. Studies of 39,000 potsherds gathered from a strata cut at El Risco (Mayer-Oakes 1959) have provided not only empirical evidence for a study of typology and seriation, but also specific evidence about the nature of the class structure and occupational specialization during a particular period in the ancient Valley of Mexico. Rather speculative extensions of these empirically-based interpretations suggest broader problem-formulations and interpretations, which are being used as a stimulus for more specific data-gathering problems (Mayer-Oakes 1960).

Additions

Having described, questioned, and criticized Sears' article, I now want to add a modest bit on the same general topic. Sears has in fact stressed an approach to archeology which can be called "functional," or perhaps "behaviouristic," since he is explicitly concerned with using the facts of time-space archeological descriptions to explain the workings of ancient societies. In pointing out the class-structured nature of certain North American societies, he has touched on the core of a problem that is much more apparent—and much more obviously neglected—in complex archeological societies. I refer to the theoretical position I am developing in recent field work in the Valley of Mexico. If the archeologist assumes homogeneity to characterize his ancient society, certain problems become important and certain kinds of interpretations follow. If, on the other hand, he assumes heterogeneity and complexity to characterize his dead society, other kinds of problems become important (on both the descriptive and the interpretive level). Sears' suggestions imply the acceptance of this position of heterogeneity. I suggest that when an archeologist knows he is dealing with a civilization he must abandon the traditional time-space descriptive goals at the outset, and create a completely new set of basic factual needs. These are tightly related to several levels of societal interpretation. For example, our unpublished 1960 test digging of a small isolated mound (locally called *tlatel*) on the west shore of Lake Texcoco (near Tlaxpetlac) produced a sample of Teotihuacan Classic-period ceramics. The nature of the site (a debris-pile island in the shallow lake) implies a kind of local social group as yet unreported for this theocratic civilization. Its distance from the ceremonial center (across the lake

and more than ten miles away) implies something about the settlement-pattern relationship. The presence of a few distinctive ceramic types (Thin Orange, for example) allows us to classify this roughly as a Classic-period occupation related to Teotihuacan. But the nature of the community looks very much like that of the adjacent and surrounding *tlateles* of a specialized Post-Classic occupation. Tulpelac looks like a Classic-period community of people who specialized in extracting salt from the shallow lake waters.

When we are dealing with what we know to be a complex society (civilization or near-civilization), it seems to me that we must make a number of assumptions about the kinds of archaeological evidence we expect to find and the complexity of this evidence. Here I disagree with Sears; but I feel this is because he does not claim to be discussing complex societies *per se*. I think we should assume a qualitative difference between simple and complex societies, use this as the heuristic basis for conceiving new approaches to the study of ancient civilizations, and then apply these approaches, some of which are outlined and suggested by Sears.

Adopting this attitude toward complex society archeology, we see that modifications (essentially generalizations) of the specific techniques Sears stresses are needed. The community-pattern analysis becomes extremely complicated when "inter-areal" patterns are added. This forces us to formulate and use a more precise, and also more flexible, definition of community. The interest in "ceremonial" structures needs to be expanded to an interest in any kind of "specialized" structure. Burials and grave goods can make a specific contribution toward understanding religion, ceremony, and specialization of labor; but other sorts of specialized deposits, such as pottery kilns, market places, cottage-industry sites, and lower-class residence zones, all help enlarge our understanding of the specific economic and political complexities of a given civilization. Basically, the analysis of disparate lots of materials must aim at relating them on a functional, social basis.

The factor which relates the attitudes and procedures that will usefully expand our knowledge of ancient North American societies to the attitudes and procedures needed to learn more about Nuclear American societies is a process shared by all. For lack of a better term, I call this "urbanization" and conceive of it as a dynamic factor affecting most ancient societies. When we deal with societies on the Formative level in the New World, we are clearly concerned with the early stages of this urbanizing

process. At the Classic level, we have urban societies of one sort or another. The more precise definition and analysis of the urbanization process I see as one of the most important tasks for archeologists today.

By T. MIKAMI☆

I was very much interested in Sears' article, and for the most part I agree with his opinions. I believe that archaeology should, through the study of sites and remains, reconstruct past ways of life, establishing social structure as well as tracing the course of its change. In this respect, Sears' opinions are, in principle, identical with mine; and it is very gratifying that many North American archaeologists share these views.

Although I am not well acquainted with North American archaeology—my field is Northeast Asia—the conditions which Sears depicts apply equally well to the work of Japanese archaeologists in Northeast Asia, including Japan. Many of them are concentrating only on the topographic study of archaeological remains (chiefly earthenware), stratigraphic study of sites, and the formulation of cultural patterns for each district. Although indispensable as basic research, these studies will not serve the purpose of archaeology without further study of social systems and their changes; and this, as Sears says, requires the aid of anthropology and ethnology. For archaeologists working in Northeast Asia, a historical analysis of the Chinese documents concerning the late Neolithic and Chalcolithic peoples of this area is especially essential. With the help of these documents, it will be possible to elucidate not only the social and political systems, but part of the religious organization as well.

However, I should like to know more exactly what Sears means by the term "religious system."

By ALAN P. OLSON☆

One can hardly take exception to Sears' thesis that the growth of theory in North American archaeology has been something other than a steady and consistent development. From the body of borrowed and adapted theory that is available, several new and encouraging developments in synthetic applications are emerging. It is obvious that, in order to derive the necessary inferences to contribute to the various levels of process, culture-historical integration, or socio-religious synthesis, archaeologists will need a broader base and a more skillfully forged body of theory on all levels of application. The current attempts to produce theory

based on archaeological needs, and with a clear view of archaeological potential, may silence the accusations that archaeologists merely adapt from related sciences, or borrow theory from other anthropological disciplines with hardly a thank-you.

Among the facets of aboriginal life that archaeology can probe, the skillful reconstruction of socio-religious practices will present one of the most vexing problems. Sears definitely advances recognition of some of the more promising areas of investigation, and suggests the results that may be expected from thoughtful inferences grounded on competent field work. Limitations, in all honesty, are not avoided either. With these ambitions there can be nothing but agreement.

It may be unreasonable to remove the argument from the level of processual interpretation or culture-historical integration, which is Sears' final goal. It would appear that there should be a firmer statement of the means to recover data amenable to the final synthesis which will allow us to make vital and valuable statements of process. Sears is hardly unaware of this factor, but his article is oriented toward higher levels of integration. Let this not be taken as a criticism of the sort that contends that every scholarly essay should be "all things to all men," and should treat every problem fully, to every reader's satisfaction.

What methodological and theoretical equipment, then, must the archaeologist bring to the field? What biases will best shape the design of excavational procedure?

It is obvious that there will never be a complete and perfect solution to the gathering of data; what may well serve one complex of inferences might be of no value in another. It is enough to say that the student should be cognizant of the most recent advances in theory and should tailor specific field problems in regard to these. Limitations imposed by time, personal aptitude, and the ever-present monetary situation will always be present, and will shape the results of excavation as surely as advances in theory. A pragmatic approach, which will produce re-usable results under the above conditions, is necessary.

On the level at which field work is conducted there is another important aspect. Even if every field project is wisely formulated, and those in charge are apprised of contemporary theory and trained to operate within it, everyone is not suited to make the utmost out of his material; and it may be suspected that, in many instances, synthesizers are born and not made. In such cases, it is even more important that the data be recovered and reported in such a man-

ner that others are able to make further inferential extensions.

Excavation techniques, as mechanical procedures, have reached a rather static level. Granting that our technology is providing improvements which will expose a greater area or more artifacts in a given length of time, no startling or revolutionary methods of excavation have been proposed in the past few decades. The shovel and the trowel may follow where electronics lead, but we cannot yet leave these tools behind.

Various sets of inferential conclusions are derivable from specific classes of data. These, upon examination, may lead to interconnections of these inferential fields and eventually to a sounder reconstruction. Recording should be slanted toward discovery of the functional aspect of archaeological data, and optimum use should be made of the relationships between various classes of material objects. If the same meticulous effort that is now expended upon ceramic and temporal studies is broadened to include the relationships of all classes of data, we shall have reports that are usable in this regard.

A further problem is that of communication among archaeologists. The personality of the archaeologist, as well as his academic personality, always obtrudes in the material reported; and this poses a problem in standardization of the final results. There is no need for a uniform code of presentation, but some thought should be given to publishing data in such a manner as to allow other students to use it for problems other than the one for which it was originally designed.

Archaeologists, a cautious lot, are usually loathe to submit many of the formulations that shape their final results. While it is embarrassing to be the author of a statement that is later changed, some of these ideas and speculations might lead further or stimulate others. The various options in recording that Sears mentions illustrate one of these problems. There should be more full, precise statements of the testing and rejection of field hypotheses. Perhaps there should be a greater effort to list alternative reconstructions. Awareness that the recording has been inadequate cannot arise after the data have been separated from their original context.

By WILLIAM A. RITCHIE☆

Among numerous constructive points contained in Sears' article, there is one on which I especially welcome the opportunity of commenting. This concerns Sears' statement that the interpretation of archaeological evidence should, as far as possible, accompany the excavation of the site or feature. In

accordance with a problem-centered procedure, working hypotheses should be formed and tested as the investigation progresses; and consequently, the methods of collecting and recording the data will vary with the needs and conditions of the particular situation.

I share Sears' doubt

that more than a small part of the potential reconstruction will ever be possible from the usual meticulously-kept records of artifact and burial location, the scale drawings and photographs, and from analysis of the specimens which are removed. There are always a great many phenomena that could be observed and recorded.

Many of these phenomena are minute clues which only the trained eye can catch on the spot and interpret. (A precisely parallel case is offered by the investigations of the criminal detective.) Therefore, I question whether limiting field work to the collection of raw data for interpretation later on (sometimes years later, when the trail has grown cold and the interest-stimulus has weakened), can be construed as good procedure.

I also have misgivings concerning archaeology by "remote control," in which the director of excavations visits the site only occasionally (if at all), but subsequently authors the interpretation. The need for actual, on the spot participation and direct observation by the person responsible for both the gathering and the scientific use of data, seems clear to any experienced field archaeologist.

By WILLIAM C. STURTEVANT☆

This paper is further evidence of a growing *rapprochement* among the subdivisions of cultural anthropology. Social anthropologists are becoming increasingly concerned with temporal factors (although as yet the time-depth in their studies is relatively shallow, and archaeologists are becoming increasingly interested in the reconstruction of functioning cultural wholes. But there is still plenty of room for further awareness of each other's results and needs. Particularly in the use of comparative data for interpreting archaeological remains (Sears' third category of analogies), it is desirable to investigate ethnological (and other) sources with more care than is usually used. Analogies may be very useful even though not "human wide." Simply because "most of us . . . do not consult *Oceania* in the ordinary course of events" (Willey 1958), significant parallels are often overlooked.

Discussions of settlement patterns and their sociological significance

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would profit from an examination of the typologies and generalizations reported in many studies by ethnologists and geographers. Chang (1958) has used some of these materials; Willey (1958) has pointed out one significant study; and many others can be found in the literature—Schwarz (1959) gives a convenient partial summary, with extensive bibliographies. The value of comparative evidence is emphasized by Geddes' (1957: 29) comment that archeological evidence of the modern Land Dayak longhouses could well lead to an inference that this society was characterized by

a system of clans under strong chiefs . . . [whereas] the fact of the matter is that a Dayak longhouse is not a long house. It only looks like one. It is in reality a series of houses separately built but joined together. The mode of Dayak life, far from being the very opposite of our own, is in some ways its apotheosis.

Curiously enough, Childe (1956: 55-56) was "tempted" to infer, from the resemblance between the longhouses of the Kayan (neighbors of the Dayak) and those of his First Northern Farmers in Europe, that the latter "were inhabited by the same sort of enlarged family or clan." For another example, see Lévi-Strauss' (1958b: 158) interpretation of the settlement pattern of the Poverty Point site in Louisiana in terms of the settlement patterns and social structure of the Bororo (especially), Winnebago, and various other societies in South America and in Indonesia.

I agree with Sears that a thorough search of the ethnographic literature, including mythological materials (Lévi-Strauss 1958c, gives an interesting example from South America), should yield much evidence to assist in interpreting the "Southern Cult." Of course the literature on the Southeast U.S. should be searched first; but it is also likely that better interpretations of this, and of other archeological phenomena (e.g. Horizon Styles) that are sometimes thought to represent rapidly-spreading religious movements, would result from an examination of the ethnological literature on revitalization movements (nativistic movements, cargo cults, etc.) not only in North America, but also in Melanesia, Africa, and elsewhere. There are numerous recent papers on the typology and theory of such movements (e.g., Wallace 1956, Worsley 1957, Andersson 1958, Mair 1959, Smith 1959, Wallace and Voget 1959).

Sears mentions the content of artistic products as a source of data; the style of the art may also eventually yield inferences as to social organization, personality type, and perhaps other aspects

of the culture. Much careful comparative work is necessary before we can be certain of this—but some intriguing efforts in this direction have been made (e.g. Lévi-Strauss 1958a, Wallace 1950).

It is also true that ethnologists often do not pay sufficient attention to archeological results and needs. A recent article by Foster (1960) reports an ethnographic study of the life-expectancy of pottery, but further studies in other parts of the world are needed to answer Meggers' and Evans' (1957: 25–26) call for ethnographic data to aid archeologists in estimating settlement duration and population size from the density of archeological sherds refuse. Ethnographic studies of pottery making would frequently benefit from a greater awareness of the descriptive features normally used by archeologists. A recent discussion of revitalization movements emphasizes the importance of "deprivation" resulting from even very indirect effects of European culture (Aberle 1959), but overlooks the existence of archeological materials that have sometimes been interpreted as evidence for such movements centuries before European contact.

By MARIAN E. WHITE*

Sears' article will play an important part in increasing the awareness of problems which can be investigated by a functional approach to archaeological data. This summary of the social and religious inferences which have been made, and the analysis of kinds of data which can be useful for such interpretations, provide a guide for specific hypotheses that can be tested in many areas. Other hypotheses come to mind for limited areas, especially where extensive ethnohistorical material is available; but these will need to be formulated on the local or regional level. (For example, a number of separate cemeteries at Iroquois villages may reflect clan organization.) This article should stimulate such formulations.

[1] I am less certain than Sears that excavations directed toward such limited problems as chronological position must necessarily destroy evidence that would be useful in the investigation of more complex problems. If the aim of excavation is the recovery of full information from those areas excavated, then the awareness of problems other than the one guiding the selection should lead to the proper recording of data pertinent to functional problems as well. In fact, excavations directed toward limited problems can give important clues to the future selection of excavations for problems of functional interpretation.

[2] The inference of social stratifica-

tion from burial mounds is debatable. A formal class system depends on an economic system which can produce and manipulate enough food to free some of the population from food-producing activities. Thus stratification is related to environmental potential as well. In regions where social classes were clearly present in the ethnological cultures, the environmental potential and economic system must have been sufficient; and similar interpretations for archaeological cultures in those regions may be sound. But in regions (such as the Northeast) where no social stratification is documented and the environmental potential was low, can one distinguish formal social classes from individual status differences on the archaeological evidence of burial mounds or ceremonial structures? Could stratification be "restricted to ceremonial context only" when it is based on the economic system? Or may this same kind of evidence indicate part-time or full-time religious specialists as well?

[3] Another example of the reconstruction of ceremonial paraphernalia and religious ceremonies is that of Adena shamanism from animal masks (Webb and Baby 1957: 61–71).

Reply

By WILLIAM H. SEARS

The comments in general are very welcome additions to my own views about the various problems touched upon in this brief article. Many of the questions raised in the critical comments have been answered, or at least discussed further, by other commentators. I shall take the position of the character in western U.S. legend who throws a bottle over the swinging doors into the bar-room and then remains outside to watch the ensuing fight in safety.

There are a few points, however, upon which I might comment further. White and Fairbanks appear to think that reconstructions of social, political, or religious systems are only interesting speculations and thus unsuitable for further testing and inference until we understand the potential of the relevant environment and the technology used to exploit it and to structure the culture's economy. Certainly there is a great need for environmental and technological studies to permit the most complete cultural reconstructions possible. But the problems involved are, if not separate, at least readily separable. We can study religious structure without knowing the density of deer per square mile.

A social or religious system can be, and often is, studied on its own terms; and I see no reason to exclude systems that have been reconstructed from archaeological evidence. The existence of such a system demonstrates the existence of the degree of technological mastery of the environment necessary to support it. To study religious ceremonialism at Kolomoki, we do not need to know whether the agriculture was based on corn or peanuts, or whether cultivators used hoes or digging sticks. Nor, if we knew, would we necessarily know more about the religion; in fact, we might possibly learn more about the agriculture from a study of the religion than the reverse. I agree that social and religious systems are ultimately related to technological patterns, and that these in turn are intimately connected with ecology, but I seriously doubt that structural studies at the level of social and religious form and function must start with the ecology and work up—even though all of this information and more, is needed for full understanding of a culture.

Haberland has, I think, missed my major point in his discussion of field techniques and whether archaeologists should or should not dig at all in certain situations. My point, which is reinforced by statements in most of the comments, is that we can shift our aim to simultaneous, interrelated excavation and interpretation, designed to elucidate social and other problems of process. Data on chronicle and distribution, welcome as they are, are then accepted as secondary. As I stated, I think archaeologists are not likely to destroy data above the chronological sequence of pottery types by excavation aimed at ceremonial reconstruction; the reverse is more probable.

In reply to Mikami's final question, I can only provide what I hope may be a working definition of a religious system. In archaeological practice, a religious system may be considered to be manifest in those reconstructed ceremonies that appear to have had supernatural functions: the accessory paraphernalia, including objects and costumes; and the physical structures which served a function in these ceremonies. From the association of these discrete parts with each other, in the total social setting as reconstructed, one may infer a pattern or system of beliefs and practices characteristic of specific, spatially and temporally limited cultural units. These culturally specific patterns, I think, may be properly termed religious systems.

Mayer-Oakes raises several interesting questions. My answer to his first question is implicit in the discussion of the necessary sequence of study and interpretation. Problems of process can-

not even be perceived until a certain amount of properly interpreted data about chronicle and distribution is available. The answer to this question, as Mayer-Oakes demonstrates, depends on the level of information. In some areas and time periods, the shift can be made now. In other areas, or in different time periods, a great deal of surveying and test-pitting is still ahead of us.

My reply to Mayer-Oakes' critical statement is, I think, implicit in my entire approach to interpretation as set forth in this paper. I cannot say that one excavates and records in a certain way to reconstruct a funeral ceremony from a burial mound. But I can say that if the goal is ceremonial reconstruction rather than artifact recovery and stratigraphy, the techniques of excavation, interpretation, and recording will follow. In North America the Indians have, insofar as the flexibility of the archaeologist permits, set the pattern. As a ridiculous, but unfortunately not quite hypothetical example, one should not use a vertical slicing technique for a pit-house; both the floor plan and stratigraphy can be recovered by more flexible techniques.

I believe I generally agree with Mayer-Oakes' "additions," which really relate to the foregoing paragraph. The entire approach to field work and interpretation must, as far as current anthropological theory permits, be conditioned by the materials one works with, from culture type to artifact class. Another way of saying this is that there is no substitute for the use of the scientific method, applied through the theories and bodies of information which have been developed by cultural anthropology and through the techniques which are part of the stock in trade of a trained archaeologist.

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An Experiment in Exhibition

"The Palaeolithic Age" in the Hungarian National Museum

by *László Vértés*

A PERSON who is designing an exhibition could hardly find a more thankless task than displaying archeological remains of the Palaeolithic Age. The layman finds palaeolithic tools monotonous and insipid; and the flaked flint tools themselves are unable to arouse even an aesthetic response. In areas such as Hungary, where palaeolithic remains do not include art or human fossils, it is hardly possible to fill even a medium-sized showroom without monotony.

These difficulties increase if the designer wishes his arrangement to demonstrate something more than simple archeological divisions and chronological sequences—to indicate the relationship of man to his natural environment, for instance; or the dependence of technology upon the raw materials available for tools. He may wish to show how Palaeolithic man laid the foundations of a social environment, forming such institutions as the family, clan, or tribe, with their corresponding religious concepts. The peculiar dreariness of palaeolithic remains makes it difficult for a specialist to indicate these relations and processes without vulgarizing his theme.

Moreover, it is almost impossible to force visitors to view an exhibition as it was conceived by the designer; or to grasp his conception and follow his theme through without interruption. An observer's attention is naturally diffused, his interest being drawn by particular displays or objects. Only rarely, and almost by chance, can objects be presented in such an interesting and readily comprehensible arrangement that they not only attract attention for their own qualities, but also demonstrate natural laws or phases of social evolution.

These considerations influenced the present author when he was asked by the board of the Hungarian National Museum to plan an exhibition of the palaeolithic material discovered in Hungary. Selecting several important problems of emergent man and primitive society, we decided to arrange the



EXHIBITION ROOM

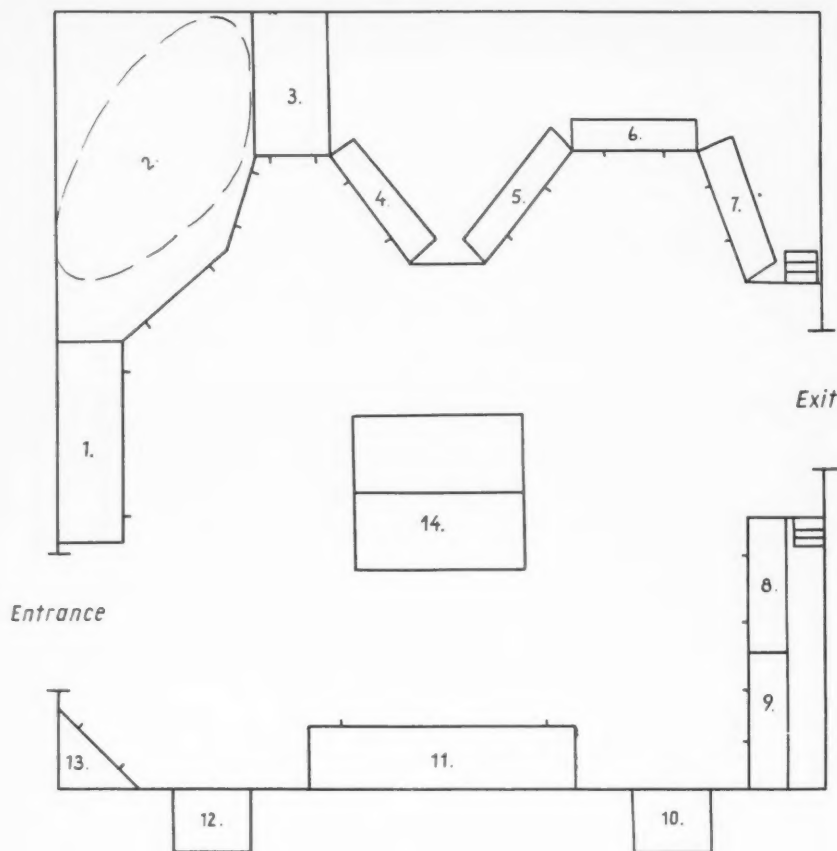
objects at our disposal into groups clearly defining their relation to these problems as well as demonstrating the chronology. The form of presentation was to be one compelling the spectator to follow our thoughts without feeling this to be irksome or feeling constraint. Our problem, therefore, was to achieve an exhibition that would, above all, be entertaining.

Since our exhibition was to be the first unit in a series of halls that would eventually present the richly varied archeological past of Hungary from the Palaeolithic to the early Middle Ages, we approached it as an experiment the ideas and techniques of which could be adopted for the other units. Consequently, we ventured to use "tricks" and electrical devices to an extent that would be out of proportion for an exhibition concerning an archeological period with more varied material.

The exhibition is housed in a room thirty feet square, in which we installed a zigzag row of showcases for best utilizing the space. In the center of the room,

BEHIND THE SCENES





a 2½-foot high and 6-foot-square block of "East Gravettian" loess from Ságvár, taken *in situ*, ensures circular traffic. Most of the showcases are 7 feet high. The lower edges of their glass windows are 2½ feet off the floor; their upper edges 5½ feet. Forward-sloping or curved glass windows that could have eliminated reflection could not be obtained, but we reduced reflection by installing windows that slope backward.

The captions above the windows are made of 1 inch thick plexiglass letters, 3 inches high, mounted on plates of frosted glass and illuminated from the back by mercury-arc lamps (15–30 W) with sunshine spectra.

The skirting and walls of the showcases are painted Pompeian red and dark golden-yellow; their frames and interiors are of naturally finished wood. Above the cases, the room is painted a dark greyish brown, gradually shading into a formless white area at the top of the arched ceiling; this provides a reflecting surface. Flat reliefs of human figures in Levante style decorate the sides of the showcases. On each wall is a fresco cave painting, in original size and approximately original colors, reproduced from enlarged photographs of paintings in the caves of Lascaux, Pech-de-Merle, and Altamira.

A dim, somewhat cave-like effect is given the windowless room by its lighting. Continuous illumination is provided by the captions, the *in situ* block from Ságvár, and the two dioramas described below. Lights inside the other showcases are operated by push buttons mounted flush with the sides of the cases.

SHOWCASE 1: THE PALAEO-LITHIC AGE

This 6 foot long showcase is divided into three sections, labelled *Time*, *Space*, and *The Living World*.

In the lower part of the *Time* section, a mechanized model, representing the movements of the earth and sun, shows the changes in the earth's orbit that were probably responsible for the Ice Age. Reproductions of the most important Ice Age deposits are displayed on three tablets mounted above the model. Above these, the course of the Ice Age climate is shown by a so-called climatic curve: a thin, weakly fluorescent neon tube whose background—in the "cold" sector, fir trees on a blue background; and in the "warm," foliaceous trees on an orange ground—makes the climatic changes readily perceptible.

The *Space* section contains maps of the northern hemisphere, of Europe,

and of Hungary, showing the circumstances and extent of maximum glaciation. Human and animal figures of quarter-inch plexiglass are placed on the three maps, which are of transparent stained glass.

At the top of the *Living World* section, a diagram shows the role of Ice Age rodents as indicators of climatic conditions. The background of the diagram is a plate of frosted glass, to which were fastened figures of voles, painted in transparent aniline colors.

Below the diagram is exhibited the only Neanderthal fossil in Hungary, the lower jaw found in the Subalyuk cave. To emphasize its importance and value, the jaw is placed on a glass bridge in the spout of a frosted glass cone (length, 2½ feet; front diameter, 1 foot; rear diameter, 4 inches), recessed in a black background and surrounded by a luminous red frame. The jaw stands at right angles to the spectator. It is illuminated from the back and top with 0.5 W lamps, whose light surrounds it with an aureole.

Captions for the objects in Showcase 1, like those throughout the exhibition, are printed in black antiqua letters of 1–2 pica size on cellophane foil which is mounted with celluloid cement on frosted glass plates. We thus obtained an effect of the text's being printed directly on the glass.

The model of the earth and sun is continuously illuminated by "black light" radiation from a mercury-arc lamp Osram (HQV 300, 75 W), whose rays are transmitted by mirror projection onto a glass plate mounted on the exterior of the case, about 2 inches from the wall. The plate shows, in luminous paint, information about the exhibition and the names of those who took part in its preparation. Because its letters shine brightly without a visible source of light, the effect of the plate is striking. Other tubes and lamps in the showcase light up when the push button is pressed. This also sets in motion the motors of the model. The electrical apparatus, which is regulated by a control unit consisting of a time relay and a magnetic contactor, operates for 60 seconds before it is automatically switched off.

SHOWCASE 2: THE FIND

This case contains an ancient hearth, weighing 175 pounds, taken in one piece from the Istállóskő cave. The 9 foot by 12 foot combustion area, on which soot, charcoal, burnt bones, and flint tools are indicated by small white arrows, is surrounded by pieces of rock; the background suggests a rock wall. Continuous illumination for the hearth is provided by side light from three concealed tubes.



"The archaeologist excavates this . . ."

SHOWCASE 3:

WHAT WE GATHER FROM THE FIND

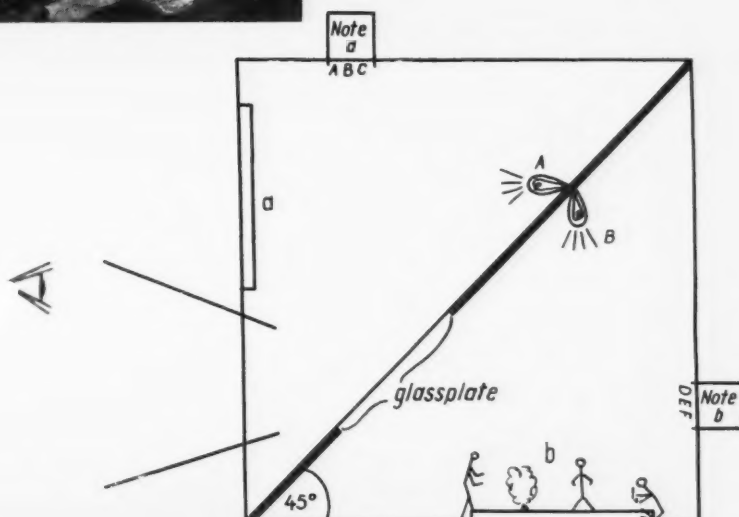
A set of inward-slanting wooden ribs surrounds a horizontal viewing aperture $1\frac{1}{2}$ feet long by 1 foot high. When the button on the side of the case is pushed, there appears a $2\frac{1}{2}$ foot by $1\frac{1}{2}$ foot plaster-of-Paris model of the hearth in Showcase 2. Above the model is the text: "The archaeologist excavates this . . ." After five seconds, human figures begin to appear, translucent at first but gradually becoming more substantial.

" . . . and he gathers this from it"



while red embers light up in the hearth, and the text changes to: " . . . and he gathers this from it." The figures are very abstract—merely suggested—and the scene seems dynamic and full of movement. The archaeologist is shown to have conjured something alive and real from the lifeless material he excavated.

The mechanical arrangement of the showcase is as follows: Behind the viewing aperture, a glass plate is mounted at a 45° angle. Above the plate is "a," the original model of the hearth, illuminated by light source "A." For its background, the wall of a cave has been indicated by a few brush strokes. Model "b," with human figures and illuminated by light source "B," is placed be-



hind and beneath the glass plate. Its background is the same as that of "a"; only their superscriptions differ. Five seconds after source "A" is switched on by push button, it is gradually extinguished by means of a resistor; simultaneously, source "B" gradually lights up, causing the reflection of model "b" to appear slowly as reflection "a" darkens. The electric control unit responsible for the lighting effects consists of two time-relays and two magnetic contactors. These start a 24 V motor which, through a high transmission ratio gear, moves a scanning arm through a circular path on which its contact runs over contacts connected to the taps of a resistor, lighting up "B" and extinguishing "A"; and at the end, a limit switch used as the final contact turns off the current.



SHOWCASE 4: HE IS CALLED MAN

This showcase has two sections: a frosted glass plate in a 3-foot-square frame, mounted on the rear wall; and the floor of the case, used to display the objects exhibited.

When the spectator approaches the case, he sees the word "Who?" written on the glass plate in luminescent blue letters, eight inches high. Under this, a red luminescent text reads: "He who makes and uses implements consciously, is man."

Both texts are written with luminescent paint that is colorless and transparent in normal light. The first text is printed directly back of the glass, the second on a narrow transparent glass strip. They are illuminated by a concealed "black light" source (Orion 250 W) which is continuously alight.

When the button is pushed, this light is cut off by a shutter closed by a magnet. Pictures, in transparent paint, of an engineer, a Papuan, a Neanderthal man, a peasant, and a Cro-Magnon man appear at 2 second intervals on the glass plate. These are encircled by an illuminated red line, and a text appears in three stages (at 1 second intervals): "All of them—are called—men." Next a circular revolving door swivels out from the back wall of the showcase. Fixed on the door is a small wooden monkey and the next: "This, on the other hand, is a monkey." The monkey is illuminated for 2 seconds by a condenser reflector positioned opposite the showcase. Then all the light sources go out, and six compartments on the floor of the showcase are illuminated one after the other. They contain such implements as a slide rule (the engineer's

This showcase has a similar arrangement. When the button is pushed, a short animated color cartoon is projected onto a frosted glass plate by an automatically controlled projector placed behind the showcase. The cartoon shows a primeval man working with his hands and feet to wrench bark off a tree. In the background, on a simplified clock face, the minute hand goes around once. Meanwhile, primeval man stops his work and sits down to make a stone implement. He then returns to work at the tree. This sequence is followed by a full-face view of triumphant early man, as he takes out the larva from beneath the bark. The cartoon caption reads: "He has saved 55 minutes: his life has become longer."

The 21-foot animated cartoon is pieced together into a continuous loop. Film adhesive was used to splice the film, and the join was reinforced by several layers of transparent adhesive tape which slightly thicken the film. Before this bulge reaches the projector, it passes under the contacts of a finely-adjusted limiter, compressing the contacts and automatically stopping the projector.

When the projector stops, objects displayed at the bottom of the showcase are illuminated and a text appears, explaining that these are the raw materials from which early man made

tool), an obsidian spear point (the Papuan's tool), a bronze sickle (prehistoric peasant tool), etc. The sixth compartment is empty, and, its caption reads: "The monkey's tool."

The mechanism is as follows: Both surfaces of the glass plate mounted on the back wall are frosted. Behind this is a transparent glass plate on which are painted the five figures and the text, which are invisible before the button is pushed. In an arrangement of tin cells placed behind the painted glass, light sources are mounted in such a way that each lamp illuminates only one

SHOWCASE 5:
IMPLEMENTS SAVE TIME

It takes an hour for primeval man with his bare hands to extract an edible larva from under the bark of the tree.

It also takes an hour for him to make a stone tool. But from this time on he peels off the bark in only five minutes.



tools. They include (1) the various kinds of flint and the tools prepared from them; (2) bones, antlers, and mammoth tusks, with the objects made from them; and (3) transparent plexiglass forms representing a forked piece of wood, a tanned skin, and the horn of a wild goat—materials used for tools that have been destroyed by time.

figure. The red circle consists of 70 red 0.5 W radio dial lamps. The whole unit, which has 18 operation periods (1 = the "black light" shutter; 2-6 = the five human figures; 7 = the red circle which also has 70 periods; 8-10 = the text; 11 = the appearance and illumination of the monkey; 12-17 = the 6 compartments containing the tools; 18 = restoration of the original situation), is controlled by two sets of equipment consisting of telephone relays timed by the pulses of an electric clock: the first set controls the red circle, and the second the rest of the unit.

SHOWCASE 6:
MAN MAKES HIMSELF

The subject of this showcase is the cephalization of man. On the glass plate at the back, a dim lamp illuminates the figure of a crouching, unarmed man. When the button is pushed, a text appears beside him, pointing out that his arms and feet are feeble, and his body is less powerful than those of the animals living in his environment. Next, caricatures of seven animals, including a tiger, woodpecker, and horse, appear around him at 2 second intervals. Their texts read: "It has teeth," "It has a bill," "It has fins," etc. After the circle of animals is complete, the human figure is replaced by another, with similar contours but showing a monster with wings, fins, hooves, a bill, etc.; it is represented in a rather antiquated manner, reminiscent of an engraving. This figure is accompanied by the text: "*If man grew all the specialized organs of the animals for himself, he would be a monster.*" After 3 seconds, another text appears at the top of the glass plate: "*He does not grow them, but makes tools for himself.*" The figure of the monster disappears, and outside the circle of animals a circle of tools takes form at 2 second intervals: near the eagle an aeroplane; near the mole, a spade; near the fish, a boat; etc. When the entire circle has been lighted, the lamps remain on for three seconds. As they are extinguished, lights are turned on to show objects representing the most important basic tools of the Palaeolithic Age: a blade, chisel, scraper, bone spear-head, etc. These objects are illuminated for 40 seconds, and then the original situation is re-established.

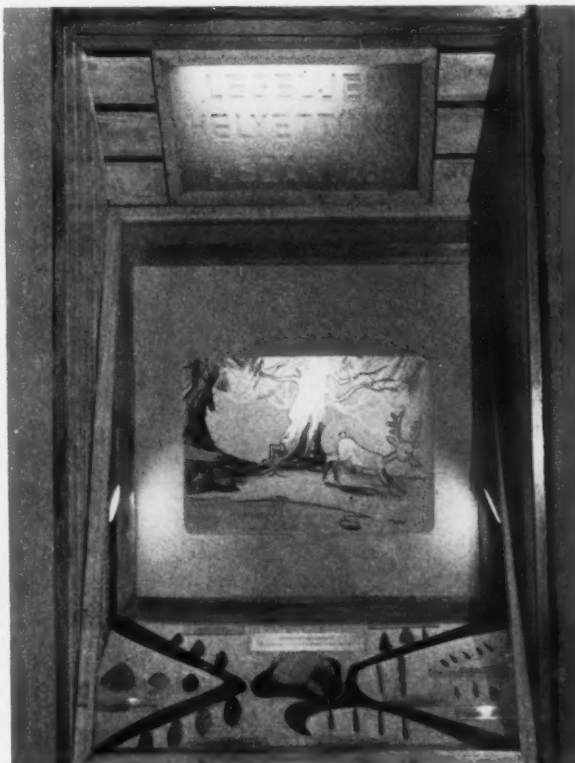
The mechanism is the same as that used for Showcase 4. The relay control units were manufactured, to our detailed designs, in the experimental workshop of one of our electric firms.

SHOWCASE 7:
LET THE DEER GRAZE INSTEAD OF US

The surface of the glass plate that faces the spectator is smooth; its reverse side is frosted. On the smooth side, a deer grazing before a forest background is pictured in transparent paint. When the button is pushed, an animated cartoon is projected from behind onto the glass plate. As the first text appears—"The deer grazes for a long time before its weight increases a little"—the mouth of the deer, and the grassy meadow before it, are surrounded by a red line, and its hindquarters by a blue one. There follows this text: "Several weeks' grazing equals one pound of flesh." The texts disappear, and a figure of early man armed with bow and arrow appears. He shoots an arrow at the deer, while the text "*One arrowshot is worth more than many months' grazing*" is projected on the picture.

Then the film is automatically switched off, and the lighting for the objects displayed in the showcase is turned on. These are Palaeolithic tools ranging from the earliest hand-axes to Mesolithic arrow-heads. In the center of the display is placed a life-size plastic brain (a school visual aid), whose text reads: "*By means of hunting, man obtained the albuminous food which is very important for the development of the brain.*" The other captions briefly give the name and age of each implement. At this showcase, the spectator is captivated by the projection of pictures onto a painted background. To heighten this effect, the glass painting is weakly illuminated from behind by diffused light at the beginning of the film. The lights of the text are turned out during the projection, just as in Showcases 5 and 8, so that they will not disturb the projection.

This showcase has the same kind of machinery as Showcase 5.



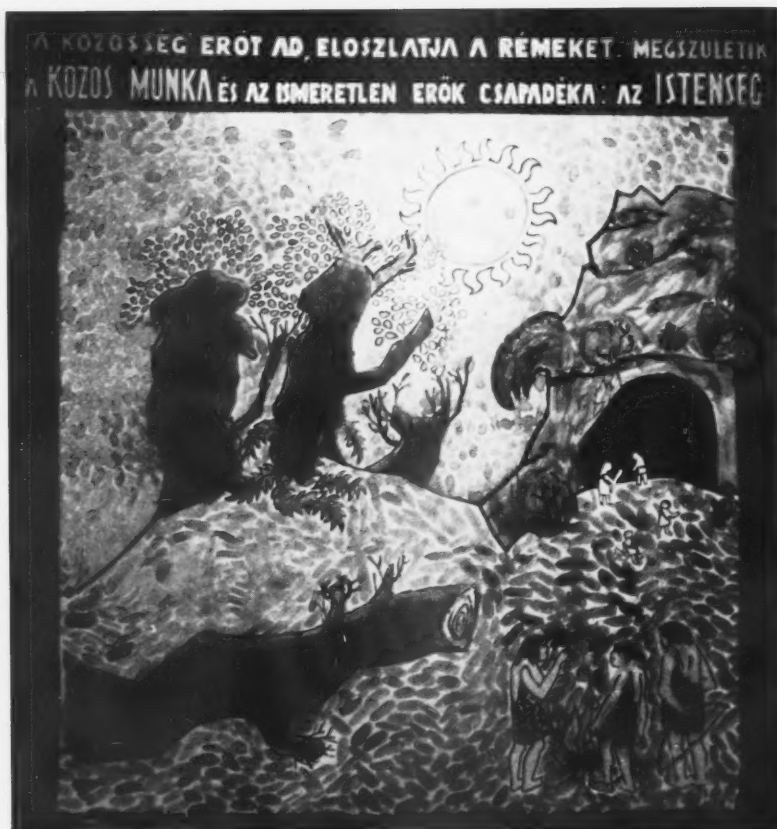


SHOWCASE 8: THE INDIVIDUAL'S
 WEAKNESS GIVES RISE TO SOCIETY
 AND RELIGION

On the smooth front of the frosted glass plate facing the spectator, is a picture in colorless luminescent paint, which cannot be seen before the button is pushed. This act extinguishes the lights of the text and opens the shutters above a continuously-lit 250 W mercury-gas discharge tube concealed beneath the showcase. An UVIOL filter permits transmission of only the ultra-violet rays, making the pigments of the picture luminesce brightly (in this case, transparent organic paints were used). The picture shows a lone unarmed man, with terrified bearing, who is surrounded by hostile anthropomorphic trees and rocks and frightening monsters. The illuminated text reads: "Man was surrounded by the unknown forces of nature. He did not understand them and was afraid of them."

After 8-10 seconds, the ultra-violet light is cut off by the shutter, while simultaneously another light transilluminates the second picture, painted with aniline colors and placed behind the glass plate. This picture shows the same landscape, but the trees and rocks are not frightening and instead of monsters there are bushes and fallen tree trunks. The atmosphere is cheerful and reassuring. In place of the lone unarmed man, three men are talking before a fire, while several other human figures are busy in the background. The text is: "The community gives strength, driving away phantoms, and society evolves as a result of collective work. The place of the phantoms is taken by religion." This picture is also visible for 8-10 seconds before lighting is turned on for the objects at the bottom of the showcase. These are cult relics (reproductions of statues, amulets, etc.), and objects associated with the institution of chieftainship and the division of labor. In the center, a 12 inch by 8 inch aperture affords the view of a rock crevice about 32 inches deep, in which the ritually buried skull of a cave-bear is exhibited (Istállókő cave). The objects are illuminated for 40 seconds.

The lighting effects of this showcase are controlled by two time relays and five magnetic contactors.



SHOWCASE 9:

THE DIARY OF LAYERS

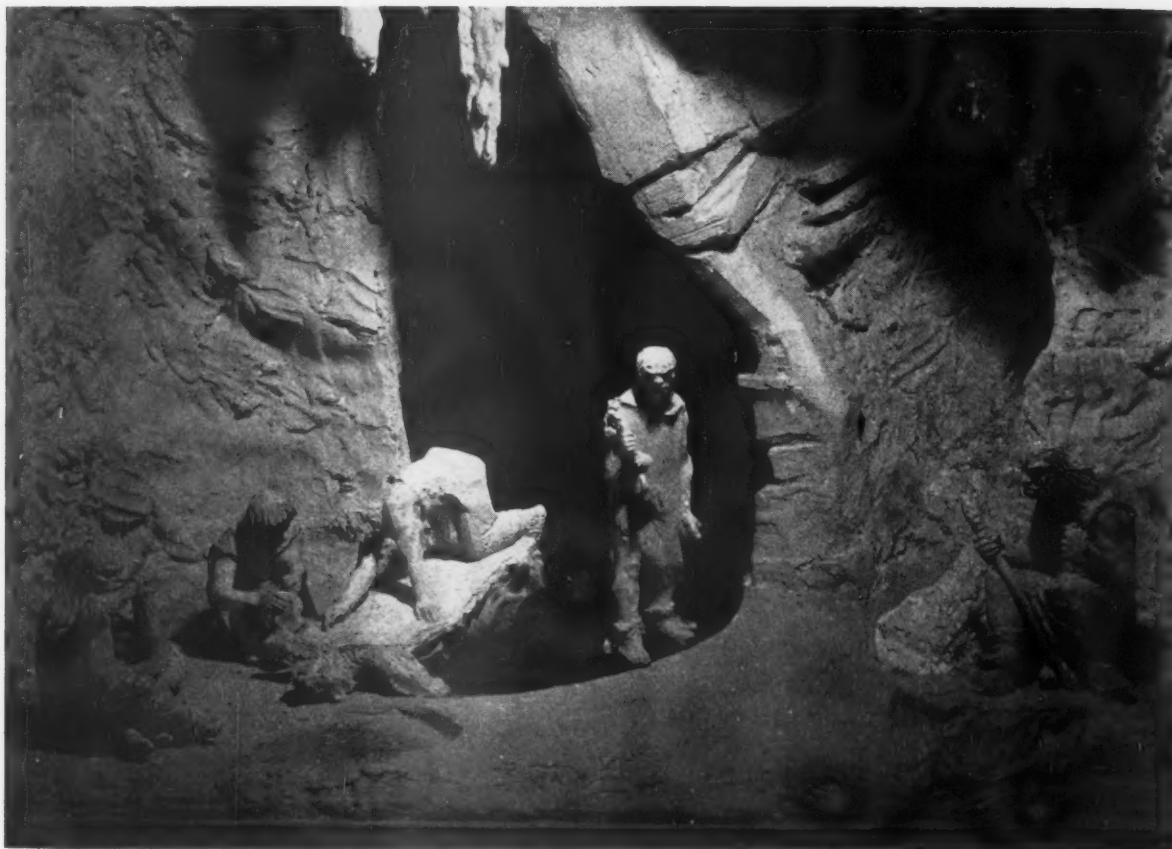
This showcase is arranged somewhat differently. Behind glass stands a large book (5 feet by 2 feet) bound in red leatherette. When the button is pushed, the cover of the book slowly opens, turning through an angle of 180° and revealing a white page of the same size. The text explains that we shall see the successive layers of a cave exhibited as reproductions of their original state. It explains the concept of a "layer," and tells why the four parts of the successive layers are called a diary of layers. This page is turned over after 25 seconds. The next page says that the upper part of the successive layers exhibited is a humus soil, deposited at the beginning of our geological age, which contains animal bones and pieces of prehistoric earthenware vessels. This layer—that is, the upper 12 inches of the reproduction mounted on the background—becomes visible when the page is turned over, since the fourth page is 12 inches shorter than the preceding pages. On the reverse (the even-numbered side) of page 3 is a landscape representing a forest—a climatic picture corresponding to present-day conditions. Pages 4, 5, and 6 turn over at 10 second intervals. Each page is shorter than the preceding one, so that larger and larger sections of the successive layers become visible. On the even-numbered side of each page is painted a climatic picture corresponding to the deposition period of the layer opposite. Under the present-day landscape is a picture of the end of the Ice Age; in the successive layers, a yellow clayey layer containing flintstone splinters corresponds to this period. On the back of page 5 is pictured a steppe; to this, there corresponds a cave loess deposition with yellow fossil



bones emerging from it. Finally, the reverse side of page 6 represents a composite forest of the interstadial period; and in the lowest reddish-brown layer corresponding to it are flintstone splinters and cave-bear bones. For 10 seconds the viewer can see all the successive layers and four-color climatic pictures. Then the "diary" is closed and the light goes out.

The mechanism operates as follows: the motor, which rotates a concealed cylinder with pins, is controlled by a unit consisting of two magnetic contactors. The pins operate a bank of keys which, when pressed, sequentially

release the bottom edges of the pages resting on them. The pages are opened mechanically by weights tied to their downward tongues with fishing-lines invisibly carried under the showcase. The last pin of the cylinder compresses a plate provided with two contacts, which stops the cylinder's rotation, and at the same time starts the motor of the closing mechanism mounted above the showcase. This mechanism simultaneously closes all the pages by means of a rotating arm and a belt. The edge of the last page comes up against a limit switch when it closes; this switches off the control unit.



SHOWCASE 10: THE EARLY MAN OF THE SZELETA CAVE

This is a continuously illuminated model. In the cave surroundings, realistic statuettes of early men show the ancient caveman's life.

SHOWCASE 11: THE PALAEO-LITHIC FINDS OF HUNGARY

Here the Palaeolithic archeological remains found in Hungary are shown in chronological sequence.

In the center of the case hangs a map of Hungary, on which several small incandescent electric lamps indicate the provenance of the finds on exhibit.

The map is painted with opaque paint on the reverse side of a plexiglass plate bent like a scroll, and is given side illumination by ten small lights attached to the wooden frame by which it is held. In this way, the colors of the map are vibrant and almost seem to light up.

Illumination of the map and showcase is turned on by push button and lasts for 60 seconds. It is controlled by a time relay and by one magnetic contactor.

SHOWCASE 12: THE REINDEER HUNTERS' SETTLEMENT OF SÁGVÁR

This continuously illuminated model shows the life at the Ságvár site, from which was taken the *in situ* block exhibited in the center of the showroom. The statues are realistic and the background is a diorama prepared in the same style. The three extremely different styles were used for models in the exhibition precisely because its nature was experimental.

SHOWCASE 13: READ—LEARN

In the last showcase, which is smaller than the others, are scientific and popular books dealing with the Palaeolithic Age, especially with that of Hungary. Its interior illumination is continuous.

The 14th unit of the exhibition is the Ságvár *in situ* in the center of the room, which was previously mentioned.

The board of the Museum prepared questionnaires to give spectators an opportunity to express their opinion of these new methods of exhibition. The several hundred forms so far received indicate that reactions among all strata of society are rather uniform. Almost all visitors, and especially teachers, welcome the light, sometimes rather humorous, treatment given serious content. They approve the use of push buttons to enlist viewer participation. Only 1½ to 2% of the spectators considered the exhibition not serious enough and preferred traditional methods. Most of those who completed the questionnaire asked us to organize future exhibitions on similar lines. On the basis of this public opinion survey, therefore, we may consider our experiment a success; and in future we shall continue to use this dynamic method of presenting ideas in "doses," through a definite sequence.

More on Cross-Cultural Education

JULES HENRY's "Cross-Cultural Outline of Education" (CURRENT ANTHROPOLOGY [1960] 1: 267-305) was published as an experiment in self-selection of commentators, and Associates were strongly urged to submit post-publication comments to appear, with a rejoinder from the author, in a later issue. The Editor received comments from F. Sierksma, Murray Wax, and Evelyn Wood, which are reproduced here. Bernice Kaplan, who used the article in a course for professional educators on "Anthropology and Community Education," sent Henry a summary of her students' reactions, which he briefly describes in his Reply.

Comments

By F. SIERKSMA

1. Henry's article is an admirable and important piece of work with many original and penetrating insights. I hope that the author will write a handbook of cultural anthropology with education as its central theme.

2. Once more empirical science confirms the partly philosophical, partly biological analyses of the nature of man by Helmuth Plessner (*Die Stufen des Organischen unter der Mensch*) and Arnold Gehlen (*Der Mensch und Urzeit und Spätkultur*). As CA attempts to pool the findings of anthropology, these authors should not be forgotten, as their work is of great importance.

3. Precisely because in several places Henry stresses man's paradoxical or dialectic nature, it might be asked whether there are sufficient data to conclude that there is no evidence that children will not lose interest in learning when it requires work. Man's dual nature accounts for his regressive, as well as his progressive tendencies. It seems unlikely that natural maturation should be the only progressive tendency. Since fieldworkers only incidentally describe such self-evident phenomena as the child's wish to grow up and its joy over being almost grown up, Henry's conclusion at this point seems one-sided. Possibly it is only in cultures where the emotional and/or intellectual stress is too great that social maturation as a spontaneous process distinct from natural (biological) maturation is absent.

4. If man is a status seeker, then the human child is also a status seeker. And then we would have a strong motive for social maturation as an innate tendency. In that case (and in my opinion Henry is right in defining man as a status seeker) "imitation" is not a residual category. Thorpe's definition

(W. H. Thorpe, *Learning and instinct in animals*, 1958: 122) may, with man, be extended to the effect that it is intensified by symbols.

5. The statement that the model of human behaviour is the innate releasing mechanism as postulated by Tinbergen is doubtful. As its name implies, the IRM is meant to explain innate behaviour. But animal behaviour, acquired by learning processes, also tends to persist as a fixed pattern, although learned behaviour is, of course, much more easily "forgotten" and modified. But the point is that the aim and the result of both innate and learned behaviour is a fixed and "filtered" universe. In this respect, attention might be drawn to the fact, well known to animal psychologists, but insufficiently studied, that aberrant behaviour evokes aggression in individuals of the same species. In animal and human life, the need for conformity is basic; in the last resort, it is the need for orientation.

6. As to education through arts, this question arises: is not the aesthetic appeal (impressiveness) of works and forms of art in itself a strengthening factor in the transmission, stabilization, and continuation of a culture? The child learns to dance; but dancing (and enjoying it) is learning part of his culture. The child learns the meaning of a mask, but the aesthetic appeal of the mask helps him to learn the lesson.

7. As a Dutchman, I was pleased to read that the Dutch in Europe and America constituted a marked exception to the rule that women should not have an education. Henry's proposed correlation between "our" women's status and their commercial role is made plausible by the similar situation in Ibo or Tchambuli society for instance.

By MURRAY WAX

Both in the past and in the present, much of formal education has been a

cross-cultural transaction. The teacher has represented one cultural (or civilized) tradition, and his students another. Research and writings by anthropologists have seldom been directed toward this transaction and, as Henry notes, when they were, it was usually to emphasize its deleterious consequences for the integrity of the recipient culture. Yet the cross-cultural educational process has been, and continues to be, so widespread that it is unwise to neglect it or dismiss it, as Henry does, as "a chapter in the history of education" (*italics mine*). Some of the variety of situations involved will be evident in the following sketch of some of the significant dimensions of analysis of the cross-cultural educational process.

1. Military or political superiority of the group either giving or receiving instruction is an important dimension of the educational transaction.

(a) The teacher may represent a people that although politically subject is considered to possess a special knowledge. In the Mediterranean world of classical antiquity, Greeks (Hellenes) were hired as tutors to the children of their overlords. Aristotle gave instruction to the son of the Macedonian conqueror of his native city. Later, other Greeks served similar functions for Roman families, teaching philosophy, mathematics, and gymnastics.

(b) The teacher may represent a politically independent people, co-equal to the recipients of his instruction. During the European settlement of the Atlantic seaboard of North America, the independent Cherokee nation consented to have missionaries work among their people, providing that they established schools and imparted the secular knowledge of the newcomers. During the 19th century, members of the Russian and English nobility employed French governesses to instruct their children in what was then considered to be the international language of diplomacy and civilization.

(c) Finally, the teacher may represent those who are militarily or politically dominant. Much missionary education has occurred in this type of situation, as in the Spanish conquest of the Indian nations of Central America. Another variant is the problem of the large modern nation-state that attempts to bring about a cultural and linguistic homogeneity among its people. The politically dominant group will attempt to make its language the national language. Thus, in the settlement of the U.S.A., the speakers of English defeated their European and native Indian rivals, and so theirs became the national

language. More recent immigrants to this country were pushed and persuaded into educational institutions where they and their children learned the language and customs which had been English, but were now considered to be general American. A few groups, because of strong political influence in a local region, or sheer religious tenacity—e.g. the Germanic-speaking Amish settlements in Pennsylvania—have managed for varying periods of time to escape the dominant English-language educational system.

2. Another dimension of analysis is the extent to which the content of learning is drawn from the specific cultural background of the teacher or is derived from the general heritage of human culture and civilization. Does the missionary among pre-literate peoples concentrate on teaching arithmetic and reading, or on persuading them to adopt European notions of modesty or dress? Whether wittingly or otherwise, the teacher in a cross-cultural school will tend to communicate many of the customs, prejudices, and predilections of his people; but the difficulties of communicating may entail some peculiar and selective learning by his students.

3. A final dimension for consideration in this brief sketch is found in the attitudes of both teacher and student toward the effect of education on personal and ethnic identity. Either or both parties may regard education as an avenue by which the student loses his original identity and becomes transmuted into something else. This is the "melting pot" ideology of the U.S.A., in which the school system has been seen as a means of blending people of diverse ethnic stocks into generalized Americans. The alternative perspective is that which regards education as a means of realizing the potentialities inherent in the student's peculiar ethnic background; thus the Kikuyu who attends an English university becomes a political leader of his people, rather than a generalized British intellectual whose origins happen to be African.

I hope that this brief discussion may serve as some sort of corrective to the traditional ethnographic practice of so esteeming the aboriginal native culture as to ignore and condemn the fascinating process of cross-cultural education.

By EVELYN WOOD

As long as change towards increasing complexity affects large numbers of people in a given society, that society can be regarded as in the process of development. There are, of course, societies in decline (e.g. those of many North American Indians), the reverse of development. No society is entirely static,

though periods of comparative stasis do occur. One such period has afflicted India, where at least 300 million people are now in the first stirrings of a renewed dynamism.

The terms "under-developed" and "over-developed," which would seem logical words to describe different societies at different levels of cultural change today, are not in fact used as polar extremes. The latter term is never seriously used at all; but it might be attached as a label to countries in which most people suppose that the development of their culture is just one stage ahead of those obtaining in any other country. The term "over-developed" would thus seem to be naturally applied by exceptionally well-informed and intellectual persons to many countries which are usually labelled "under-developed." The People's Republic of China is a ripe example.

Fortunately, there are a certain number of more or less uninvolved observers, who have lived long enough in both under- and over-developed countries to be able to make some kind of objective pattern out of the relative value of these two labels. It would not, of course, be reasonable to expect a citizen of the Soviet Union to make an objective comment on the United States, even if he had lived there for a year or so; the reverse situation is even more unlikely and would equally limit any possibility of objective description with regard to the level of development reached by the North American culture (or should it be cultures?).

It is doubtful whether United Nations valuations have much authority which is accepted. The specialized UN agencies, at any rate, by and large seem to adopt American standards. Their social science justly comes in for a good deal of professional criticism—in the U.S.A. as well! This comment, of course, is strongest when UNESCO is in mind.

Henry's analytical article on education starts a very apposite lead-in to the problem just outlined. Any reader who wishes to get the most out of Henry's highly suggestive paper would of course have to go much more deeply into the actual problems that he and his colleagues have investigated; and it is a little difficult to see how this could be done without actually witnessing some field work similar to that which has been reported. In any case, Henry's paper is concerned almost exclusively with drawing lively comparisons between elementary school-teaching in the United States and the corresponding procedures in a few other cultures, ranging from studies of aboriginals made in different parts of the world, including North America, to the early ideas about running a school which were current in

the (then) American colonies of the British Hanoverian kings.

It is probably the innocence of the present commentator about the refinements of both elementary education and anthropology (where the latter study seeks to define personalities in terms of cultures) that causes a certain dissatisfaction with the rather complicated matrix of ideas which Henry sets up as an "Outline."

Nevertheless, this very extensive and subtle analysis of the aims and methods of education sets into motion a great number of acute questions with respect to the kind of development which is supposed to be official development policy in India today.

Community Development and Freedom

Only one positive statement can be made about what may be called the "internal liberation movement" within India. It was doubtless assumed by the politicians—mostly Western-educated—who worked to secure India's independence from the British Empire that the majority of their country-folk, living in over a half-million villages, were equally dedicated to the same concept of "freedom." However, in the thirteen years since the political independence of India has been a fact, we have all seen far more detailed power settle itself in the hands of the new, indigenous rulers; while the majority of the people living in the villages have appeared to lose their enthusiasm for "freedom."

In justice, one must add that India's rulers are conscious of this defect. Consequently, they have recently introduced measures for "Democratic Decentralization," a program designed to speed up local self-government and put both planning and its implementation into the hands of rural people. The detail and the pace of the takeover will vary from state to state within the Union, and the acts now being passed by state legislatures carry varying degrees of safeguards and restrictions of peasant power. The new system is popularly called *Panchayati Raj*.

The Concepts of "Freedom," "Culture," and "Democracy" in Modern Usage

As the quotation marks suggest, the word "freedom" has recently come into such wide and constant use as to lose definition; "democracy" and "culture" are other once-valuable terms of the same debased currency. All three words now require redefinition. Henry's paper does a most important part of the job for "culture" by resetting the word in its proper place: the over-all system of values in a society which he has studied, which has major interests for an anthropologist.

The usage of "freedom" today is perhaps the most extraordinary. Americans and Russians both use it to describe their own condition as distinguished from that of the other. India's political freedom was gained by action in which peasants worked and took risks in common with their westernized, intellectual compatriots. The resultant freedom secured by the peasants cannot be so simply reconciled with the complex of real social and economic freedoms as these appear to the intellectual leaders of the struggle. This effect is far more marked now that the latter have become the rulers, and the peasants have become the millions to be ruled.

The real political independence of India would appear to extend only to its government, as this is formed by quinquennial elections based on universal adult suffrage. Other institutions, with a longer history of making *ex parte* decisions for millions of people, have also persisted and in fact have expanded within the new political system. The law, the military, the financial, and the civil administrative services are not in practice entirely subordinate to the parliament; each has its sectors of free decision. In theory, of course, most areas of free choice can be abolished by the order of the Minister, when he has the backing of his colleagues in the Cabinet. Further still, any of the parliaments can theoretically force a Minister to give such an order restricting the area of free decision. In practice, this democratic control will work with the civil services, but not much with the law or defence. We must accept the fact that even in the civil services there will almost always be some area of free decision to which no new ministerial rules have yet reached.

Problems Faced by Rulers in a New Democracy

Perhaps this constitutes one of the core-problems in forming and maintaining a viable new Welfare State. There will always be exceptional individuals among the rulers—both politicians and officials—who have more or less solid backing, from the groups which contain them, to make the very new decisions which are required. Such "strong men" are able to confront situations which will affect the lives of millions of people, for which there are no clear precedents that have crystallized into rules.

1. *Making decisions.*—Clearly, there often has to be a means of making a choice of action to be taken by other people. There are only two ways of making such a choice.

The first way seems to be more commonly practised in the totalitarian states: it is to refer the possible ways of

action to a body of principles, in which one is confident that the answer will already have been fully worked out by theory, in anticipation of just such an emergency. The simplest short label for this would be the "religious" (or "dogmatic") method. One must remark here that the dogmatic method, in its purest form, is steadily becoming less popular.

The second method of making brand-new choices is founded on one negative principle: that no situation in human affairs ever has a precise precedent; nor have previous assemblies of human experience had the capacity to imagine all the possibilities that flow from one choice or the other, since it is not possible to observe every contributing influence at the point of the new human situation. Consequently, this method of "rational choice," which about half the world claims to follow, depends upon the laborious collection of evidence which has to be weighed before any choice of action can be determined.

It is the helpless trend of every Welfare State, whether totalitarian or otherwise, to take upon itself increasing regulation of individual lives. No highly-educated person (and in modern states, probably few of the others) seems to be really satisfied with either of these systems of making choices. We are all inclined, however, to accept the system we know, which usually embodies a modification of one or the other of these different methods.

A justified criticism of both methods is that too many people spend far too much time in taking an ineffective part in public affairs. Thus, the people in a totalitarian state must spend a major part of their time collecting and checking evidence so that planning by the authorities can proceed smoothly. On the other hand, states with a parliamentary system occupy far too many people in the free discussion of policies and decisions. The collection of evidence in a non-totalitarian state is just as onerous—only it is assigned more often to paid workers. Much of the factual material is never used by the parliament or its committees; the cabinet, of course, is concerned with principle rather than detail.

2. *Provision and use of scientific evidence.*—The only thing which is new in the situation of apparently large differences between political, and hence cultural, "freedoms" is the very real capacity to analyse what goes on. In this latter context, the non-totalitarians probably have considerably more hard experience. But how far is such analysis used? The scholars who should be contributing much more to the problem of how to use the dynamic structure of a given state are the historians. Somehow there seem to be more difficulties about

their collaboration with other social scientists than arise with the analysis of briefer periods.

Henry's study reports a fruitful collaboration among teachers, psychologists, and anthropologists. This is excellent, as far as it goes. The dynamic element is perhaps not adequately supplied for most of us by cross-reference between "modern cultures" and the primitive societies which have been studied by field anthropologists; yet that is Henry's method.

We all know, as a matter of family or broader local history, that the reconciliation of interests among politicians, officials, and the people whom they claim to guide has been a constant process of adjustment. Social anthropologists would probably confirm that this same dynamic compromise has been followed by all the settled forms of society of which we can obtain records.

The Problem of Shaping a Welfare State

So we come back to our own problem: how to form and maintain a dynamic and viable welfare state. We have first to face the historic need for reconciliation among three other sets of interests: the individual; the local group to which he belongs; and the larger groups, regional or national. The superior capacity for fine analysis, which is a product of the modern, rational, and Western world (whether totalitarian or otherwise), must of course be used to provide reliable indices. However, such fine methods consume a lot of time and superior manpower. They should therefore be sparingly and thoroughly done.

There does not seem to be any overwhelming reason why fine social science work should be able to reach any better solution to the individual's need for partial subordination to several groups than has been reached in the past by much cruder methods. In fact, many of the solutions of current problems may already be at hand, if we would look for them. For example, countries like India, where millions of people have reached group-solutions without too much mutual destruction throughout the whole of recorded history, ought to be a rich quarry.

We do need more micro-studies here. Preferably they should be done by teams consisting of a sociologist and a cultural anthropologist, at least one of whom must be a fairly good psychologist. An economist as adviser would be a great help. Perhaps the same teams, or some of them, could do the macro-studies which are much more badly needed. The overweighting of economics thinking in all the macro-studies done so far in India urgently needs correcting. Within a team that does macro-

studies, there must be a communications specialist.

A Sine qua non for Any Studies in India by Social Scientists

It is of no practical value to make any studies of the types just suggested unless there is first some assurance that the results will be used by the governments concerned. This proviso applies with particular force in India. We have here several excellent institutions doing substantial research, mainly of an economic or demographic character; a fair number of political studies exist; but we are weak in the other social sciences. Our historians, of course, have the most unsatisfactory material with which to work.

One of our newer sources of macro-studies is the Programme Evaluation Organization of the Planning Commission. This was set up along with the official Community Development Plans, and is applied solely to check their implementation, in some considerable detail. The PEO could do better with a great deal of supplementary aid from other bodies of investigators. It might be a good test of the Indian Government's real intention to use the evidence produced by the new studies now proposed, if it were first to agree to accept supplementation of the PEO's work by aid from outside social scientists. A first, urgent need is to examine the Communications aspects of Community Development and the National Extension Service which operates its plans in the countryside.

It is very plain to this writer, who is a communications specialist, that this Service is terribly lacking in controlled communications. The block to acceptance and use of social scientists' evidence lies with the governments of India. Putting it bluntly, once one turns the searchlight of modern scholarship on the political situation in India, one must inevitably wonder why the politicians seem inhibited from obtaining and using objective facts before they begin to talk freely about huge plans. There are honourable exceptions, of course; but on the whole, too much large-scale action is taken without sufficient evidence having been collected and weighed.

Perhaps there is only one solution to this last small problem: to rank "talking big" as a psychological need to which the people must partially adjust themselves. There seems no good reason, however, why social scientists should make such adjustments.

India's Problems as Shared by the Rest of the World

In any case, this particular problem, which has been rather laboriously out-

lined in the preceding paragraphs, is clearly common to the whole world. All that can be suggested here is that there may exist, within India, a peculiar wealth of diverse knowledge which could lead towards solutions. But to reach practical solutions, macro-studies must enable a process of early synthesis rather than the apparently endless analysis entailed by the method presented by Henry and his colleagues.

It is uncomfortably clear that only very different solutions can be found which will satisfy groups of so many hundred millions of people. Historical evidence from outside India may not be of much use here. At no stage in history, for instance, could one think of more contrasting solutions than those reached in People's China and U.S.A.

Possibly the only historical analogy would be that which may have appeared to render the systems of the nomadic hordes of Central Asia completely incompatible with the system followed by the new Islamic societies or the Holy Roman Empire of the 13th century. The historian is of course likely to say that co-existence was a simpler possibility at that period of social and economic development, particularly in respect to communications. But we may rescue one thought from the analogy: that there is no need for the new system of conformist values which is clearly coming into being in India to follow either the Russo-Chinese or the Euro-American model. A third system, drawing some elements from both its neighbours and having a majority of elements rooted in its own past, seems to be at least equally possible in India.

Henry's analysis of the way in which people grow into a life of freedom in the United States must be useful to those who are seeking to define the nature of any possible freedoms in India. It does not seem likely, however, that many of the possible comparisons among the forms which freedom takes in North American cultures will help to define the forms which are desired in India. Henry's study is, of course, much too restricted to reach the definition we need in this country.

Even though history is not well recorded in India, fragments are clear enough to show the kinds of lives which are—or were—considered to embody good values. Many of these values appear still to be current standards, at any rate of the ideal man and his life.

Examples of the Good Life in India

What is consistently thought to be admirable in the vast field of Indian tradition may firstly have been exemplified in the lives of certain families—not only the families in the very limited periods of power which happen to have

been reflected by segments of the Indian imagination, but also the locally recalled families or individuals whose lives are considered to have been a good influence on society.

The suggestion has been made that the families which were admired, such as the Udaipur Maharanas in the Sisodia clan of Rajputana, were often typical of the particular region and period from which their annals are sung with veneration. But this suggestion seems unscientific. It is mainly heard in India, and it seems to stem from the unquestionably strong tendency to romanticize and glorify the past. More often than not an element of deity appears in the family tree of families traditionally regarded as models. Sisodias, for instance, are said to be descended from Surya, the sun. There is a hint of theogony in this. Yet the virtues which are admired in old families distinguished in such ways are the same as those that are admired in historic individuals. The pattern does not change much in the later centuries or over the country, within each of the four main castes. It does differ between the castes. The virtues practised are described by the traditional historians, or by such record-makers of antiquity as Manu and Kautiliya, in remarkably concise and practical terms.

Perhaps the richest quarry for "model histories" of this nature is the Mahabharata. This inspiring piece of legendary history does not conceal the fact that there could be both good and bad sides within the same large family. The moral force of the Mahabharata derives from the relative success of the good, Pandava elements.

Strictly speaking, one should not confine the consideration of models available in Indian history or legend to exponents of Hinduism alone. More accurately, if one is to regard the admirable figures in the Mahabharata or the Ramayana as Hindu exemplars, then one must at the same time open the field of Hinduism to include any of the other religions which may have succeeded in establishing themselves in India, in such a way as to live in easy harmony with the Hindu majority. On this argument, there should also be historical or legendary models derived from the Jews, Christians, Muslims, and Parsis. This latter point is only made to recall the embracing nature of Hinduism on the one hand, and the much plainer fact that no kind of social or cultural model can be really acceptable to this country unless it fits within a theocentric society.

The Different Orientation of Indian Values

How long the cultures of India will continue to make the recognition of

human power as an inferior element in the universe the centre of their philosophy, one does not know. For the present, this seems still to be far and away the majority point of view. The many different forms in which men acknowledge their subordination to powers within the universe, which are completely beyond the comprehension of the majority, need not even be listed. It is enough to remark that they range from forms of worship which are without tangible object and are of extreme sensitivity, subtlety, and intellectual compass, right down to the crudest of animistic practices, such as one would associate with remote aboriginal cultures in Polynesia, or equally untouched African tribesmen. The strength of Hinduism as an embracing system lies within its ability to contain these vastly different kinds of relationship with deity, and an apparently infinite capacity to separate those which do not suit any particular individual who may call himself a Hindu.

In order to promote education of any profundity for a majority in India, it would still be unthinkable to disregard religion as a frame of reference for every action in human life. It would, in fact, be just as unthinkable as in Europe of the period when the Holy Roman Empire flourished. This insistence on a background of good values derived from religion *does not mean teaching religion* itself. That would be equally unthinkable to Hindus today, who are avid for academic education.

The preceding paragraph may be read with equal application to all kinds of education here, except in the higher reaches of the universities. Besides school-education up to higher secondary schools, one must also think of most kinds of adult education. The religious background is especially important in extension education, through which the Community Development Plans are being implemented. With children, this necessary insistence on the good values of religion is to satisfy the parents; with adults, it is to satisfy all but the youngest, emerging from adolescence.

One must say here that the current situation is highly unstable. Government is trying to disregard religion in its schools, and discourages reference to it in the Extension Service fieldwork. The movies import an element of dissolution to the religious principle of defining the good values. Ethics are not strong in Hinduism, in any case. Therefore, this instability must be considered in any long-term design for education here. The immediate aim of education must therefore cautiously assume that the forms of Indian society in which it is being shaped are changing very slowly. Further, the values to which this

society gives expression are certainly changing no faster. But they may suddenly enter the rapids on the stream of change.

Education in India's Development

Nobody has ever considered that India could undertake her own reconstruction as a modern nation without an enormous increase in education. It is urgent for both children and adults. We progress slowly towards the aim of increasing the literacy rate from below 20%. Without this, communications are slow and expensive. The rural literacy rate is of course lower. Female literacy in Central India was below 4% at the 1951 decennial census. Yet the major task of development is among rural people, who constitute over 80% of the population. They have to move from a medieval condition into one that can reasonably match itself with the modern world which inevitably presses on them.

Community development, into which the Government has put such huge investments of money, materials, and men, is designed to enable village people to develop themselves. The chief vehicle which the National Extension Service promotes for this purpose is common action among the people of each village. We must therefore take a brief look at the recent history of these communities which are supposed to be reshaping themselves.

Necessary Research into Value Changes

Resources of time and men will not permit setting-out a program to examine the history of 558,000 enormously varied villages. What is required is a study of change over four or five generations, so that all India is roughly represented. The settled people must be tackled first. For this objective, most field-anthropologists who have worked in India seem to agree on the need to recognize about 40 distinct culture patterns.

It should be easy to get land and law records. Peasant memories are phenomenal, and will lend colour to the account. Sampling will be difficult; but one should probably aim at 5 selected villages in each of the 40 regions. This is a big enough programme, since micro-studies are necessary, and each may take a team over a year to do.

For the particular purpose of estimating the historical rate of change in values, villages with between 150 and 350 households will be about right; for different reasons, smaller or larger villages would give confused pictures. It is better to choose villages of mixed religion, where these are numerous in the culture area (Kerala, for instance). One should entirely reject villages of a sin-

gle caste. For two out of the five samples, more isolated villages should be chosen in preference to those which are easy of access.

Tribal people have already received disproportionate attention from anthropologists. They must not enter into the sample of 200 villages, but should be plotted for more gradual coverage, by anthropologists alone, of the untouched areas. For the studies of settled people, teams on which other disciplines are represented will be required.

A good indicator for choices in the main sample pattern will be that of language. Written languages appear in fourteen scripts; but writing is rare in some areas. There are, however, rich oral literatures as well. One would have to allow for covering at least twenty languages.

The immediate aim of this research should be an account of the histories of the past century, studied from the point of view of changes in values. There are some definite, practical statements which can be made about most villages, regarded as communities, however weak their social bonds may be.

1. At irregular intervals, several individuals have abandoned traditional practices, despite the opposition of their families and neighbours. Sometimes a new practice has replaced the old; sometimes not. The rebels have got away with it, and their revolt has been gradually followed by more persons, until the whole village has worked itself free of that particular outworn conformity.

2. The second indicator of change comes from migrants, individual or group. The group migrations have many causes, and can be of climatic, famine, epidemic, or cultural origin. The individual migrations have occurred for economic reasons. Both have caused change in village values, the individual effect being more slowly cumulative than that of the group. Sometimes the group has consisted of unreconciled rebels as described in the preceding paragraph.

3. A third indicator is the effective precept or example of a highly respected individual. The obvious, almost universal, agent for India is Gandhi, who injected Christian ethics into both Hindu and Muslim values. But local *gurus* (the sense of sacred teacher being loosened here) have also been effective.

The Need for Value-Changes Associated with Development

The writer believes that it cannot often be reliably forecast whether a value-change in a society must precede a development, or will necessarily follow it. Economic development provides a cause for changes in values; but other kinds of development may essentially follow ac-

ceptance of new values. The adoption of family planning in rural India would present the latter type of succession.

No change can be efficiently stimulated, nor encouraged to grow roots here, unless those who work at village development are ready to acquire a far better knowledge of the people. The shifting spectrum of values must be respectfully studied and recognized by urbanized Indians. To find this knowledge, teams of social scientists will be preferable; pairs may serve well. The same teams which explore the historical picture of recent change can double on the task of imparting their knowledge to extension workers.

We do not have enough researchers in the social sciences in India. We shall have to import scientists from abroad who are prepared to stay at least three years, and who will teach their Indian associates better methods of research. What countries can provide such men?

We would do better with the less rigidly objective type of social scientist. Over-elaborate techniques, such as Rorschach Tests, should be avoided in rural India. The men and women who come should also be from families which have recollections of experience in the changes accompanying development. Book-knowledge is not enough. The U.S.A. probably cannot provide many such social scientists; its natural prosperity and the new character of its social values form a difficult personal background from which to orient oneself to India. It is doubtful if any of the British Commonwealth countries can provide more than a handful of the persons required. The U.S.S.R. is probably out as a source. Italy, France, and Holland seem to be possibilities.

We may, of course, find it impossible to get enough social scientists of the type indicated. In that case, the solution might be to brigade the few we can get with young professionals in disciplines that normally do not engage in field-work in rural areas. Thus, geographers, political scientists, historians, and even scholars in the humanities could make up parts of teams.

The key to finding the right persons is to look for a live interest in the problems of rural development. A fascination with the dynamics of the adjustments among people, officials, and politicians—or between the elite and a supposedly dull mass of population—would be the strongest evidence of such an interest. Obviously, a good deal of fiddling with its wards will make it easier for the organizer of this All-India research to find and fit the key.

Conclusions

Community development is probably the worst term that could have been

found to label the processes of rural development in India. On the one hand, it sounds like bombast from the elite—as if they were going to show village people how to run the perfect, joint-action society in miniature. On the other hand, one can hardly suggest that villages in India have not been communities; they have held together longer than most peasant groups in a changing world.

Whatever the label, however, one ultimate problem of rural development is to get messages flowing smoothly in both vertical and horizontal directions among all types of communities in India. Besides the communities of village-folk, there is, after all, a community of officials in each division of organized government. The National Extension Service is the nearest community of officials to Indian villagers. Communities of politicians are called Parties; they have internal factions, too! Various orders of scientists also form communities. All these certainly have to communicate back and forth, vertically, with the villagers.

The messages must be within a controlled communications system. That is, they must be regulatable for content and tone on the vertical channel towards the villages. Feedback at junction points must be receivable for use in adjusting messages for either quality. The centreward flow up the vertical channel from the villages must be condensed by the Extension Service so that it will flow reasonably fast. Horizontal messages in rural areas must have similar controls installed.

Until we have a clear idea of the values in Indian villages, and some hints as to their dynamics, we shall not be able to design really efficient communications systems.

Reply

By JULES HENRY

On comments of F. SIERKSMA: I am most grateful to Sierksma for his remarks on my paper, and for the opportunity they give me to further expand my ideas about learning.

To me the most stimulating of Sierksma's observations are included in his Sections 3 and 4. First he asks "whether there are sufficient data" to conclude that "there is no evidence that children will not lose interest in learning when it requires work" (p. 304, of the *Cross Cultural Outline of Education*). In reply I would say, first, that this statement is amplified somewhat on page 287 of the "Outline," as follows:

While Dubois (1944) and Raum (1940) give some evidence to show that children

outside of Western culture can be reluctant to learn what they are supposed to learn, the over-all impression gained from reading the literature on non-literate cultures is that children appear to want to learn tribal ways, and that adults take the children's role as learners, and their own as educators, for granted. *But this is only an impression, for anthropology has not explored systematically the problem of the eagerness of the primitive teacher to teach, or indeed, of the primitive child to learn.* [Italics added]

While the evidence of reluctance to learn is clear in the "great" civilizations, the data from pre-literate cultures are not clear; but on the other hand, neither are the data on eagerness.

Secondly, in pre-literate culture the problem of learning has been obscured by the closely related one of performance. For example, when a Tale adult berates the children, saying, "Young ears don't listen. Can't you look after your cattle properly, you good-for-nothings, you things-with-sunken-eyes . . ." ("Outline," p. 280) is he dealing with the problem of learning failure or performance failure?

Thirdly, an apparent willingness to learn may be motivated by fear of status loss or other punishments. For example, I quoted Pettitt's work on the North American Indian:

The status of childhood is made uncomfortable by depriving him of the best foods, discriminating against him, making him bathe in cold water, morning and night. At the slightest breach of discipline, which he knows will end when he proves his manhood, his father becomes enraged and may throw burning bark in his face.

All of this, however, is merely introductory to a much larger issue raised by Sierksma, which derives from his remark that the child's "wish to grow up" is "self-evident," and from his apparent affirmation in Section 4 that there is "an innate tendency" for "social maturation."

From years of observing living children and from living in and studying the households of psychotic children, I have come to the following conclusions:

(1) There are many childish behaviors that children do not wish to give up; i.e. there are many ways in which they have no "wish to grow up," but *must*. Foremost among these are, of course, in very young children, autocratic behavior and the wish to be taken care of; but each age has its reluctance.

(2) There are many behaviors characteristic of older people that children *would* take "joy" in doing; e.g. exercising authority, manipulating treasured cultural objects, indulging in the types of impulse-release permitted adults but denied to children. In United States culture these are staying up late, smoking, drinking, going wherever one wishes, exercising authority, having a car, etc.

(3) At every stage of its life the child renounces some of its childish ways—or childish learnings—in favor of more mature ones, partly because its parents and other adults disapprove of an older child doing what is permitted a younger, and partly because

some rewards accrue also. Thus the child responds to the disapproval of its parents—and later of its peers—by renouncing young ways and taking on those befitting an older child. At the same time he collects whatever rewards may be forthcoming for performance at a higher level. The most obvious points of stress-in-renunciation in all societies center (with different emphases depending on the culture) around the biological functions, authority, dependence, and impulse control.

(4) The "joy over being almost grown-up," of which Professor Sierksma speaks, is always balanced against the pain of renunciation of childish adaptations; and *where the adult world does not offer adequate rewards in the shape of adult self-realization, for the renunciation of childish pleasures, there will be a corresponding reluctance to grow up.* The contemporary phenomena of the impulse-ridden delinquent and the sociopath are in part products of this antimony. Thus the tendency toward social maturation, which is indeed "innate," must be examined at each step in terms of (a) the primordial regressive pull of "immature" joys; (b) the punishment for clinging to them; (c) the rewards for renunciation; and finally, (d) the presence of adult models and their nature. In the extreme case, that of infantile autism, there are no adult models because the child has been isolated, so he does not mature; this is the so-called "ferral child." On the other hand, even in the presence of adult models the child's "tendency toward social maturation" may be uncertain and wavering if the adults are punitive, repulsive, over-indulgent, or confused.

These considerations raise the issue of the currently fashionable notion of the "drive to mastery." This is a mid-20th century word for what used to be called the innate tendency to learn, and like all learning, "mastery" will not occur in the absence of suitable social models or in the absence of rewards for conformity and punishments for non-conformity.

In section 5 Professor Sierksma demurs at my observation that "educational procedures have regularly taken as a model the innate release mechanism postulated by ethology" ("Outline," p. 268). Now the IRM functions in such a way that the display of a certain behavior by animal X releases, in a completely deterministic way, a complementary behavior in animal Y. I maintain that in education in classrooms, particularly in the West, when a teacher says "Oh," all the children are supposed to say, "Ah!"; and that this is the fundamental abiding quality of mass education everywhere. In non-literate cultures the "Oh!-Ah!" couple is brought about in different ways, but automatic responses of laughter, weeping, competitiveness, rage, fear, etc. can be witnessed in any of them.

On the whole I am delighted with Sierksma's reply to my paper, and only hope I have done it justice.

On comments of MURRAY WAX: In spite of the wisdom of Wax's remarks, it is necessary to say that he completely

misunderstood me. He seems to have taken my statement on p. 284 to mean that I was brushing aside the entire problem of cross-cultural education. It is very clear in the text, however, that I did not mean that at all, but rather that the problem is so broad there is no room in a paper to give it the extended treatment it deserves. The statement in question is as follows:

The issue of coincidence or lack of coincidence between the social group of the educator and of the educated of course vigorously confronts anthropology in situations of acculturation, but since it constitutes a chapter by itself in the history of education, it will be barely touched on here [italics supplied].

But Wax's misconstruction of what I have to say does not rest here. In his last sentence he says:

I hope that this brief discussion may serve as some sort of corrective to the traditional ethnographic practise of so esteeming the aboriginal native culture as to ignore and condemn the fascinating process of cross-cultural education.

I devote the entire last paragraph of Section III:8 (about one-third of the discussion) to the positive contributions of modern education to African life and to that of the Spanish-Americans in the United States.

On comments of EVELYN WOOD: I wish I knew more about India so that I could answer Wood's profound and sweeping paper with the scope and scholarship it deserves. Not all of my weakness, however, derives from "pure," but rather from "tempered" ignorance. I think I have been reasonably dutiful, as an American cultural anthropologist, in "keeping up" on the Indian studies that have been done by American and other social scientists. Yet I am perplexed when I am asked to think about the problems of Indian values, for I ask myself: How deep, in the peasant villages, and how immutable are the values of Ghandi, Nehru, the Mahabharata, and the Ramayana? Do the people really live by them, as we [do not] live by the Sermon on the Mount? And why do not anthropologist writers on contemporary India mention the *Panchatantra* as a source of values? At the University of Chicago, I knew a young man who had come from India to study economics. What he learned at his grandmother's knee was the *Panchatantra*, a zoomorphic *Prince* if there ever was one. It is true that Indian villagers come by the thousands to listen to the dramatic recitations of the great epics and to the words of the religious men. But Americans do that too—on Sunday—and we flock by the millions to see cinematic bible stories with stereophonic sound! The great lack that I find in the anthropological material on India is a real effort to elucidate, for us

in the West, the value system of the villager. What I do have is a sense of the Indian peasant's commitment to caste and village provincialism; a carefully balanced reciprocity; a desire for land, a higher standard of living, and high status; a determined, but loosening, involvement with his family; and some continued admiration (of what we still call here "the Protestant virtues") of abstemiousness, hard work, and honesty. To this might be added the rajput values of strength and vitality so much admired by that great rajput, Nehru. All of this is quite apart from religious considerations, which vary enormously in India, and which may or may not support these values. But, as I say, here in the West we need to know more, much more, than we already know about the values of the individual Indian peasant.

Wood suggests that American social scientists are disqualified for the kind of investigations India requires today—because what India needs are social scientists who "have recollections of experience in the changes accompanying development." Actually, very, very few men or women who have had such experiences are—except in countries that have undergone sudden catastrophic changes—young enough to undertake directly the kind of studies envisioned by Wood; for it takes a long time to experience such changes except where there has been revolution, and one *does* grow old. The problem of the American anthropologist is not that he cannot place himself sufficiently marginal to a cultural situation or that he has not had experience of change, or even that he does not have "a live interest in the problems of rural development." The problem of the American anthropologist is first that he is an American and therefore must watch his reporting carefully; and secondly that American anthropologists have been trained either to deal too gingerly with the issues of poverty, chicanery, exploitation, and miserable death, or to ignore them altogether. American anthropologists have worried much more than American journalists about being refused re-entry to a foreign country because of "revelations." It is impossible systematically and honestly to examine institutions anywhere without discovering the stupidity, callousness, and dishonesty that impair them, and which have been characteristic of the "high civilizations" also at all times. But who can report this of a modern political system? Only the brave—and the brave are all too few. Woods also says that

to reach practical solutions here, macro-studies must enable a process of early synthesis rather than the apparently endless analysis, which the method presented by Henry and his colleagues seems to entail.

Wood, even like us in the United States, is in a hurry. When one is running a revolution he does not stop to do research; but if one is going to stop to do "scientific" research, he cannot run a revolution. Wood is talking about *intelligence*, not about research, and I am all in favor of flying-cadres of trained people going into villages and studying particular problems swiftly. Research is a different matter: for entire nations and other types of great human problems (e.g. mental illness), *the research plan must be pitched on a foundation of from 10 to 20 to fifty or even a hundred years. Detail—"endless" analysis—*is not the issue, for we have such analysis in the economic studies of the Western world. Economists in some Western countries can readily answer many questions regarding trends in the past and possible variations in the future, because their research has been going on for so many generations, and it will continue, for it is "endless" in its details and in its time perspectives.

Let us consider a problem from another field. According to the New York Times, December 27, 1960:

The Chinese have traced their earthquake records back to 1139 B.C. after having put 150 historians to work examining ancient records. The result is a map of seismicity (or frequency of quakes) that is used in designing the new dams and other structures.

This is what I mean by a time perspective. When Norbert Wiener said that in the social sciences accuracy is impossible because our statistical runs are not long enough, he struck directly at our habits of haste.

I will now take up one at a time some of Wood's other observations, each one trenchant in itself, but not, it seems to me, of the same importance as the ones I have just discussed.

"Under-developed" is not *merely* a term of invidious comparison used by countries who consider their culture superior to others. In the United States the term "under-developed" has lately meant solicitude for peoples suffering more than we from poverty and disease, and we have tried to be helpful. That such helpfulness was mixed with a heavy measure of self-interest goes without saying; that in this world there can be anything else is problematic. That self-interest has often militated against helpfulness, we all know to our sorrow. Meanwhile our approach becomes year by year more human.

In regard to the question of freedom and democracy, it is a commonplace that every nation gives its own interpretation to these words. In India a danger is that independence and *de jure* abolition of the caste system may merely substitute one form of disfranchisement for another. It profits the

low-caste Hindu little if the best jobs and opportunities in the new occupational structure still go more often to high-caste people than to him. Nor does it profit the peasant at all if the administrative structure is manned by corrupt officials who "do him in." Such things can make one understand why the peasant might be losing some of his enthusiasm for "freedom." But who am I to be telling this to Wood? He knows it, I am sure, to the tips of his fingers and even, perhaps, up to his neck.

Such considerations raise questions about the utility of a concept like "communication." Studying a communication network in a monolingual, literate social system, one derives the conviction that obstacles to communication center largely in the problems of the system itself—primarily in what is euphemistically called "conflicts of interest," including ritualized distance or "differences in motivational level." These are jargon-words to cover the fact that people are hostile to one another at certain points or are not interested in their jobs. Compared to such factors many of the technical (not technological!) problems of communication are secondary.

Wood makes the interesting observation that

The second method of making brand-new choices is founded on one negative principle: that no situation in human affairs ever has a precise (similar?) precedent.

This depends on how closely we wish to look at a phenomenon. Law courts are run in part on the basis of cases that set precedents, which means that certain classes of events recur often enough and with sufficient similarity to make them useful as guides. The problem for decision-making is therefore not alone the perception of the variety in human events, but the selection of men and women who can be trained to detect the invariants among the variety. By the time social scientists, domestic or imported, have analyzed the data from 200 Indian villages, time will have shot past us. Such a study will, however, form part of a permanent pool of data that will be the reservoir on which all future investigations will draw.

Wood's remark touches on one of the crucial problems of education; viz. that in the elementary grades children should be trained in decision-making. For this we need teachers who will raise important moral and ethical problems and guide children in their solution. None of the solutions, of course, will be "pure," for in a commonwealth all decisions are governed by political considerations. At any rate, in American classrooms practically no training is given in this most important of human skills. It seems never to have occurred to anybody here that man must be trained

to rationality as rigorously as to the alphabet! Meanwhile it is necessary to point out that if the training we give in school in decision-making has no opportunity for practical exercise we are wasting time.

Wood says that my "analysis of the way in which people grow into a life of freedom in the United States must be useful to those who are seeking to define the nature of any possible freedoms in India." I was not aware that I had made such an analysis, but it might be a good thing to try. When one makes a beginning, however, one immediately discovers that whatever freedom exists, dwells within politico-economic values that are never brought to consciousness; for example, that industrial capitalism is the Ordained and Natural Order of the universe, and that on the other side dwell only red she-devils; or that Communism is an Ordained and Natural Order, beyond which dwell capitalist monsters; or that the caste system is the Ordained Order beyond which dwell nothing but bureaucrats. The central problem of freedom everywhere is to discover first the invisible constrictors of perception—as I implied in the conclusions to my paper. In this sense the problem of the definition of "the forms which freedom takes" is everywhere the same: *men must first be made aware of what they think.* Meanwhile, I am afraid Wood has put words in my mouth and then criticized me for having said them!

Romanticizing the past is often merely a way of validating the present. I admire the way the Serbs, after a hundred and fifty years, still glory in the memory of their exploits against the Turks; and this helps validate the new Yugoslav state, gives buoyancy to contemporary life, and knits the peasants together. I hope they never stop teaching their epics to the children! One of our problems in the United States is that we have spitefully debunked our past. In America we take pride in our capacity to despise greatness.

Wood says that

In order to promote education of any profundity for a majority in India it would still be unthinkable to disregard religion as a frame of reference for every action in human life.

And I would maintain that for a *modern* educational system to attempt to *maintain* religion as a frame of reference is unthinkable. This does not mean removing religion from the Indian scene, though heaven knows, there are millions of people everywhere whose religion is merely formal. Modern education is essentially scientific; and in that context, most of the enormous variety and quite incredible phantasy of much of Indian religion can play no part. This does not mean that compassion, morality, and love should be jetti-

soned. They should have a place in every school reader put into the hands of children everywhere.

On comments of BERNICE KAPLAN's students: In the summer of 1960, Bernice Kaplan gave a course at the University of Michigan on "Anthropology and Community Education." All but one of the 30 students were professional educators, and they were

asked to write brief papers on the article (i.e. the "Outline") covering the following points:

1. An overall evaluation of the article from their point of view as teachers or administrators.
2. Any weaknesses or omissions that seemed important.
3. How the article proved helpful, if at all.

In a spirit of truly moving dedication, Dr. Kaplan summarized these reports for me and sent me the 17 double-spaced pages. After three and a half pages of complimentary flowers the rest is all bricks. Like any good campaigner, I must say: "Thanks, for the bricks; I shall use them to build my house!" For there is much in the criticism that can be used in the development of my book. A great deal of the criticism has to do with what I left out of my outline. I shall give a few examples of the omissions mentioned by the educators:

teacher training institutions
teaching of reading
goals of our schools
subcultural groups
theories of the way children learn
education in European countries of today
parents' role in education
peer education
purposes of teaching
use of group activities as a teaching method
positive uses of discipline
use of the "discovery method"
variability of education practice throughout the country

This will give some idea of the range of topics suggested for further coverage; but there were many, many more. It is obvious that to touch on all of them would require several volumes. Considering the dimensions of CA, the time available to me now, and the enormous research task that still awaits, all I can do here is express my appreciation to the educators for their thought and to Dr. Kaplan for her selfless labor, and proceed to talk about subjects that might be of interest to my anthropological brethren and which happen at the moment to touch some of my dearest prejudices.

Many of the educators were annoyed with my depreciation of the making of valentines, pot-holders and similar busy work, for they felt that such activity had

a serious educational value, for example:

Childish handiwork is not time-killing. Such activities permit students to display their creativity and artistic talents. It also helps to improve muscular coordination. . . . These activities may be a means of gaining recognition and establishing prestige for a child who otherwise does poorly in school. Most students develop a sense of pride and joy in giving to others when they are permitted to present their handiwork to their parents. They also need the relaxed atmosphere inherent in such activities.

(The activities) develop . . . independent thinking and provide a period for getting better acquainted with the children in a less formal atmosphere.

. . . they could be developing . . . eye-hand coordination, rhythm and the ability to sense spatial objects.

It is impossible to know where to take hold of these arguments, since most of American educational theory is assertion and counter-assertion. Cronbach's *Educational Psychology*, which is one of the best books in the field, shows (inadvertently) the great difficulty in getting anything approaching a controlled experiment along any dimension of classroom learning. It seems unlikely, however, that the average American child really needs those occasional moments spent in childish handiwork to help him develop muscular coordination or to sharpen his perceptions. I would assert, in the best American educational tradition of assertion, that most of what is claimed for the "handiwork" moments is really accomplished out of school. The central issue for an anthropologist, however, is not so much the validity of such tribal arguments but rather the function they serve as tools legitimizing behavior. What we really are dealing with in these arguments of the educators is the *folklore* of childish handiwork and related activities—e.g. so-called art. In this connection I have my own anthropological experience in American classrooms to go on, and that of my students. It looks to me as if this activity serves the social function of letting down the bars: giving the children a chance to relax from the boredom and tension of their regular work, and giving the teacher a chance to catch up on paper work and to get some relief from endless repetition of the same subject matter. The anthropologist studying American culture perceives these ceremonial releases of tension in some American institutions. Childish handiwork also serves the function of affirming the importance of the "arts and crafts" segment of our American prestige system, and of giving expression to our nostalgia for the vanished arty-crafty epoch of American folk-culture days. In the presence of the devastation of our crafts by

the industrial revolution, and given the accompanying hunger for the log cabin and the crock of home-brewed liquor anybody who, like me, points a finger at arts and crafts is a spoilsport!

My critics object to our discovery that most art-teaching in elementary school is not art-teaching at all. The teaching of art, one would presume, involves familiarizing students with art traditions and with the components of art: *problems* of drawing, media, composition, light and shade, perspective etc., and some kind of systematic effort to direct the children's attention to these components as they work. Our investigations simply have not turned this up in the elementary schools. We have never seen a single art book in any elementary school classroom nor detected great works of art (in reproduction, of course) either in the hallways or in the classrooms of the schools studied. Meanwhile there are many pictures of rockets, missiles, machinery, cute animals, presidents of the United States, athletes, etc. Indeed it is impossible either to walk around in an average American school or sit through an elementary school art lesson without coming away with the impression that the febrile paw of Mindlessness rampant is clutching at the throats of our children. Art is not something that is communicated to children through art "periods" that last one-half to three-quarters of an hour a couple of times a week. Such a treatment of art merely emphasizes for the children the fact that art is unimportant. Just let the corridors and the classrooms of our schools be filled with reproductions of great art and give up the art periods altogether, for the ordinary teacher does not know how to teach art.

Let us face the fact that art is of no importance to most educators—the terrifying ugly books placed in the hands of children are proof enough of this—and go on to what is important to anthropologists: the social function of the so-called art endeavor. The current function of the art period is similar to that of the arty-crafty period—to ceremonialize tension and to give expression to those lonesome intellectuals in our culture who affirm that we must not be a nation of barbarians.

This brings us to the problem of creativity. In a sense there is no such thing as "creativity"—there is only a fog of argument. But there *are* creative people. The discussion about creativity derives from the notion that there is a *quality* called "creativity" that inheres, in a kind of Platonic or Aristotelean way, in all people who are creative. Meanwhile we see creativity destroyed by teachers while they wonder disconsolately about it. The reason teachers destroy creativity is because the

very nature of creativity is threatening to *all* teachers, whether at the elementary or graduate level. *A creative act is simply a departure from the routine and expected.* Such creative acts fall into two simple classes: the culturally acceptable, and the culturally unacceptable. When most people talk about creativity they are all mixed up between what they are willing to accept and what a creative act is: between what constitutes a creative act and what they are willing to call one.

Meanwhile the central problem for anthropology in general is the social function of the current talk about creativity in our schools. One of its functions is to legitimize art and arts and

crafts; and another is to give expression in the *adults* to those impulses that have been suffocated in *them* through their capitulations to cultural expediency. When an educator cries: "Let our children be creative!" he is merely weeping inwardly while he thinks: "Look what has happened to me!"

Finally I should like to comment on the complaint that I have not said that "teachers should have a real empathy for the children, really love them, if teaching is to be a success." This expresses the over-evaluation of love, so characteristic of our culture. It is clear that a person will do well only if he likes the task and the object on which it is performed; but to demand of a teacher

that he love children seems unnecessary, and it is very likely that much harm will be done to excellent teachers who, however, do not love *children*, though they *love to teach*. Of course, they cannot *dislike* children either, for that will destroy the effectiveness of their teaching. Again the anthropologist must ask himself: "What is the social function of the emphasis on love?" I would suggest, in the absence of any proof that the ability to love children is a critical element distinguishing between teachers of equal skill, that the emphasis on love is related to our feeling of the harshness of our culture—its callousness and danger—and the feeling that children are, therefore, being cheated.

THE 34th INTERNATIONAL CONGRESS OF AMERICANISTS

Reported by Oskar Grunow

HELD JULY 17-25 1960 at Vienna, Austria, the 34th International Congress of Americanists included 420 participants, representing 36 nations (Table 1). As at previous meetings of the Congress, the official languages were English, French, German, and Spanish.

The Executive Committee of the Congress consisted of:

President	Robert Heine-Geldern	Vienna, Austria
Vice-Presidents	Herbert Baldus	São Paulo, Brazil
	Manuel Ballesteros-Gaibrois	Madrid, Spain
	Etta Becker-Donner	Vienna, Austria
	Ignacio Bernal	Mexico, D.F., Mexico
	Emil W. Haury	Tucson, Ariz., U.S.A.
	Wilhelm Koppers	Vienna, Austria
Secretaries	Henri Lehmann	Paris, France
	Alberto Rex Gonzales	Cordoba, Argentina
	Doris Stone	San José, Costa Rica
	S. Henry Wassén	Gothenburg, Sweden
General Secretary	Anna Hohenwart-Gerlachstein	Vienna, Austria
Treasurer	Oskar Grunow	Vienna, Austria

An invitation from the Mexican government to hold the next Congress of Americanists in Mexico City was transmitted to the Congress by Ignacio Bernal, and gratefully accepted.

Since the proceedings of the Congress will be prepared for publication with as much speed as possible, the various papers, films, and reports that were presented are simply listed here according to subject, with the names of the authors provided for those interested in following up any topic. Addresses are listed only for those authors who are not included in the most recent list of "Associates in Current Anthropology" (CA April 1961).

PAPERS

Archaeology

Terminología de fenómenos y culturas cuaternarias en Suramérica.

HELMUTH FUCHS

Die ältesten vorgeschichtlichen Reste in Mexico.

P. BOSCH-GIMPERA

Carbon-dated ocean level changes offer a new system of correlating archaeological data.

KENNETH W. VINTON, Department of Physical Science, Canal Zone Junior College, Balboa Heights, Canal Zone.

Steinzeitliche Funde altpaläolithischen Typs in Chile und ein Versuch ihrer Eingliederung in eine Besiedlungsgeschichte Südamerikas.

JULIUS SPINNER, Av. 21 de Mayo 2752, La Cruz, Chile.

The Trail Creek Caves on Seward Peninsula, Alaska.

HELGE LARSEN

Archäologische Strandwalluntersuchungen auf Kap Krusenstern, Nordwest-Alaska.

HANS-GEORG BANDI (and J. L. GIDDINGS)

Seventh century evidence for the use of tobacco in northern Arizona.

ELIZABETH ANN MORRIS

Ceremonies and legends depicted on Maya pottery.

J. ERIC S. THOMPSON, Harvard, Ashdon, Saffron Walden, Essex, England.

Recent excavations at Altar de Sacrificios, Guatemala.

GORDON R. WILLEY

Excavations at Las Cuevas, British Honduras.

ADRIAN DIGBY, Department of Ethnology, British Museum, London WC 1, England.

Cave sites in British Honduras.

ALEXANDER H. ANDERSON, Arch. Commissioner, The Secretariat, Belize, Br. Honduras.

Nouvelles données sur la pénétration mexicaine en pays Maya.

HENRI LEHMANN

Preliminary report on archaeological investigations in coastal Guanacaste, Costa Rica.

MICHAEL D. COE

Recherches archéologiques dans la vallée

du Tempisque, Guanacaste, Costa Rica.

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ELIZABETH DELLA SANTA (see above)

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CARL HENTZE, Am Willgraben 7, Trautheim über Darmstadt, Germany.

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LOUISA ROWELL, Dept. of Fine Arts and Archaeology, Columbia University, New York 27, N.Y., U.S.A.

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PIETRO SCOTTI

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HOMER ASCHMANN

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WILHELM SAAKE

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ČESTMÍR LOUKOTKA, Krěnická 56, Praha 10 > Sitašnice, Czechoslovakia.

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JAMES J. PARSONS, Dept. of Geography, University of California, Berkeley, Calif., U.S.A.

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CHESTER S. CHARD

Recent Siberian migrations into Alaska.

MARIUS BARBEAU

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WAYNE SUTTLES

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ANDREW P. VAYDA

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ANNA BIRGITTA ROTH

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ERNA GUNTHER

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ALBERTO RUBIO FUENTES, Sociedad Española de Antropología, Etnografía y Prehistoria, Madrid, Spain.

TABLE 1. REPRESENTATION AT THE CONGRESS.

Country	Full Members ¹			Total	Associate Student		
	a	b	c		Members	Members	Total
Argentina	3	2		5			5
Austria	25	2	2	29	4	17	50
Belgium	2	2		4	1		5
Brazil	5	3	5	13	1		14
British Borneo		1		1			1
British Honduras	1			1	1		2
Canada	4	2	1	7	2		9
Chile	5	1		6	1		7
Colombia	1	1		2			2
Costa Rica	9	1		10			10
Czechoslovakia	1			1			1
Denmark	8			8	2	1	11
Ecuador	3	1	2	6			6
England	12			12	3		15
Ethiopia	1			1			1
France	10		2	12	4		16
Germany {East	7			7			7
West	30	3	3	36	6	3	45
Greece						1	1
Guatemala	3			3			3
Haiti		1		1			1
Holland	6	1		7	2		9
Holy See	1			1			1
Italy	2	1		3			3
Japan	4	2	1	7			7
Malaya		1		1			1
Mexico	17	3	1	21	4		25
Norway	1			1	1		2
Panama		2		2			2
Peru	3	2		5	1		6
Spain	10			10			10
Sweden	7	1		8	2		10
Switzerland	4		1	5	1		6
United Nations	1			1			1
United States of							
America	72	19	7	98	16		114
Uruguay		2		2			2
Venezuela	4	2		6	3		9
Totals	262	55	26	343	55	22	420

¹ Full members: a, individual members present; b individual members absent; c, organizations or institutions.

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GERTRUDE HAFNER, Schillkestr. 46, Graz, Austria.

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E. ADAMSON HOEBEL

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KATHERINE LUOMALA

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EVA LIPS

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WILLIAM T. ROSS

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FRANCIS X. GROLLIG (see above)

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HANS HACK, Koninklijke Instituut voor de Tropen, Linnaeusstraat 2A, Amsterdam, Netherlands.

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BERNARD POTTIER

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MARIUS BARBEAU

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D. L. OLMSTED

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JUAN COMAS

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The Pacific Area

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THOMAS S. BARTHEL

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CARLYLE S. SMITH

Various Subjects

A comparison of cultural values: Europe-America.

RITA V. LOEB, 2923 S. Sepulveda, Los Angeles 64, Calif., U.S.A.

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ALEXANDER GRIGOLIA, Ea. Baptist Theological Seminary, City Line and Lancaster Ave., Philadelphia 31, Penna., U.S.A.

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LOUIS W. ROBERTS, S. J., Stillgasse 6, Innsbruck 2, Austria.

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**Interrelationships of New World cultures—
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**The program: its purpose, history, and de-
velopment aims.**

GORDON F. EKHOLM

Report of first year's results.
CLIFFORD EVANS

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CLIFFORD EVANS and BETTY J. MEGGERS

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PAUL J. F. SCHUMACHER, 180 New Mont-
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**Erstes Zusammentreffen mit den (Paakas
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EITTA BECKER-DONNER

SYMPOSIA

**Evolución y Transformaciones de los
Pueblos Indígenas Desde la Con-
quista**

Chairman: JOSÉ MIRANDA

Participants, and their papers:

JOSÉ MIRANDA, "Importancia de los
cambios experimentados por los
pueblos indígenas desde la con-
quista."

DAVID FRENCH, "Types of organization
and change among North American
Indians."

EDWARD P. DOZIER, "Differing reactions
to Christianity among some North
American Indian societies."

WOODROW BORAH, Dept. of Speech, Uni-
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Calif., U.S.A. "Population decline
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changes of New Spain in the middle
decades of the 16th century."

WIGBERTO JIMÉNEZ MORENO, Cetzada
Norte de Gelati 12, Mexico 18, D.F.,
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el Cristianismo."

GUY STRESSER-PÉAN, "Evolution des vil-
lages indigènes de la Huasteca."

HERBERT BALDUS, "Was ist seit 1500 aus
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**Dringende Forschungsaufgaben hin-
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süd- und zentralamerikanischer
Indianerstämme**

Chairman: HERBERT BALDUS

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JOHANNES WILBERT, "Nord-Venezuela."

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JENS YDE, "British Guiana und Nord-
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HANS BECHER (see above), "Nordwest-
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ALFRED MÉTRAUX, "Région du Xingu."

WILHELM SAAKE, "Nordwest Mato
Grosso (Jurua-Arinos Gebiet)."

EITTA BECKER-DONNER, "Guaporé-
Gebiet."

KARIN HISSINK, "Bolivien (Rio Beni-
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UDO OBEREM, "Ost-Ecuador."

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**Pictorial and Written Sources for Mid-
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Chairmen: HOWARD F. CLINE, Library
of Congress, Washington 25, D.C.,
U.S.A.; and PAUL KIRCHHOFF.

Participants, and their papers:

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C. GIBSON, Dept. of History, State Uni-
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Polychrome Zapotec Tomb Paintings

Recent excavations in a new Zapotec area near San Pedro Yolo in the Sierra de Juarez, Oaxaca, have brought to light the first Mexican polychrome tomb paintings from the Historic Period. In a village with twenty terraces, family tombs below the house-foundations contained polychrome representations in three distinct styles: geometric stepped-frets; deities in the style of the Mixtec codices; and crude naturalistic representations of human figures, centipedes, and the sun. Tombs were rectangular, with slab roofs and side niches containing simple greyware bowls as offerings; each tomb contained from 2 to 8 adult burials showing dental mutilation, cranial deformation, and in one case, trepanation. AGUSTIN DELGADO

Research Reports

Sociolinguistic Variation in Modern English

One aspect of sociolinguistic variation which so far has not received the attention it deserves is social variation in Modern English. Far too many people are using this language as a means of communication to allow us to neglect the question of its "proper" usage. In this connection the following point may be made: sociolinguistic variation in Yana, Koasati, etc. is doubtless an exciting subject; one cannot but admire the dedicated efforts of those who have sought to collect and preserve—to "archive"—the various characteristics of disappearing cultures. But research becomes scientific, and its results are raised to the rank of science, only when certain fundamental or basic categories have been established and an appropriate methodology elaborated.

It is one of the tenets of modern linguistics that the study of bygone stages and periods can be successfully pursued only if it is based on what has been achieved by observation of fully-developed, living languages. Sound linguistic methodology must be based on a comprehensive linguistic theory. Obviously, both linguistic theory and methodology can be worked out only by abstraction and generalization from the varied and exhaustive materials supplied by living languages—from investigations of the properties of widely-used languages.

The bibliography appended to Bright's article (CURRENT ANTHROPOLOGY [1960] 1: 424-25) may be supplemented

by a recent article by Josef Vachek, "On social differentiation of English speech habits," published in *Philologica Pragensiy*. I am glad of this opportunity to endorse what Vachek says about the nature of the process Fischer described as "a protracted pursuit of an élite by an envious mass, and consequent 'flight' of the élite" (Bright 1960: 424). According to Vachek (pp. 224-25):

One should not lose sight of the fact that the differences in language referred to as class-indicators, though certainly of social provenience, are not always entirely social in character. At least some of the class indicators, that is to say, are being reevaluated into indicators of style: what are referred to as U-features prove to be, at least in many instances, well-fitted to signalize the higher style used in top-level intercourse, while the so-called non-U features are able to function as signalizers of lower stylistic levels, characteristic of everyday conversation, informal and familiar talk, etc.

Much of the efficiency of intercultural and interdisciplinary communication depends on the ability of scholars in different parts of the world to use English in "top-level intercourse." Miss Pikelis (quoted in Letter to Associates No. 6) was quite right when she ascribed "the relative dearth of major articles from outside [the English-speaking countries to] self-criticism of language competence," or bluntly, to the authors' reluctance to write in "poor English." But what, exactly, is "poor" and what is "good" English? Whom and what should the non-vernacular user of English try to emulate? The situation is further aggravated by the user's being vaguely conscious of the existence of an obscure something termed "British style" vs. "American style." But for the life of him (or her), he (or she) would never be able to tell which of the two he (or she) uses or tries to use, especially in writing.

"Standardization" of usage should be one of the chief concerns of modern linguistic science. So far, however, consistent and over-all normalization of usage has been confined to pronunciation and spelling, while the vast field of lexical, syntactic, and above all, "stylistic" orthoepy remains, if not entirely unexplored, at any rate not yet presented in orderly fashion. I have participated in an investigation of this problem (Akhmanova and Veselitsky 1960), which was based on the material contained in one of the best works of its kind—Fowler's *Dictionary of Modern English Usage*. A detailed investigation of all the entries in that dictionary has shown that the selection of items was not based on "categorization"; i.e. no orthoepic theory was initially worked out; the choice of items was predominantly empiric. When an attempt at "categorization" was made, the result (as presented in our article) may be

summed up as follows:

The orthoepic (or orthologic; *ortho-* being preferred because *orthoepic* is usually restricted to pronunciation) categories, which actually underlie the lexicographic collection under consideration, may be presented under the following appellations:

- (1) synonymic variation
- (2) correlation of components
- (3) "secondary reaction" to language.

Akhmanova and Veselitsky think they have succeeded in showing that practically the whole of Fowler's excellent dictionary is covered by these three main categories. For the present report, however, the following conclusion seems to be of especial importance: All these categories (i.e. euphemisms, onomasiological and "elegant" variation, novelty-hunting, pomposities and superfluous words; incompatibles, incongruous vocabulary, repetition of words, etc.; didacticism, fetishes, overzeal, pride of knowledge, etc.) "categorize" what the user of the language should *avoid*. But the non-vernacular user needs *positive* knowledge; he wants and tries to be clear and effective at the appropriate level—and usually fails. How much (or far) he is short of his aim he can only assess indirectly, through his ready awareness of the unfortunate effect of the "stylistic" (i.e. orthological) efforts of the non-vernacular users of *his* native language. Obviously, all this has little to do with foreign language-teaching in the ordinary sense. Millions of people make themselves understood in hundreds of foreign languages every day and everywhere. The great problem of "reevaluation of class-indicators into indicators of style," as viewed in the present note, is how to enable the scholar writing in a foreign language to ascertain that he is not only saying *what* he intended, but also expressing himself *as* he intended.

Reported by OLGA AKHMANOVA

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Sheep-keeping and the Shepherd in Central Europe up to the Beginning of the 20th Century

Ethnology has not unjustly been reproached for its preoccupation with the peasantry and disregard of other classes of working people. This one-sided point of view is apparent, for instance, in the

fact that even occupational groups that are intimately connected with the countryside have remained practically unstudied. This explains the romantically distorted picture of the shepherd and his existence which is still common today; accepted without question and incorporated into ethnological presentations, it has acquired the semblance of a scientific character.¹

For several years, I have been collecting material for a monographic report about the Central European shepherd in the widest possible framework, striving to place in historical perspective everything concerning sheep-keeping and shepherds, and providing a small foundation stone in the larger building of the cultural history of the working people. The result of this study will be published shortly. In the meantime, the following résumé is offered here.

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Correspondence

► I feel it would be most advantageous if C.A. and the Wenner-Gren Foundation could help bring about closer relations among individual scientists and institutes. In my own experience, Africanists, general anthropologists, linguists, etc., have far more intimate contact with other scientists in Europe than do those who study European ethnography or ethnology. As Bela Gunda and Geza Csermak suggested at the Paris conference (1960), narrow national investigations are now obsolete. In no other field of European ethnology is this issue clearer than in research on agricultural implements, where information can be obtained only from general European observations.

Therefore, it is time for an exchange of conferences among the scientists and institutes concerned. These conferences must include extensive exchange of data, and should establish rules for presenting photographic documentation (e.g. from how many sides a plow must be photographed or measured). The various topics of research should be sent to a central clearing house—probably Copenhagen—so that duplication of effort can be avoided, and a scholar can be provided with scientific data from all parts of Europe. Questionnaires should be circulated throughout Europe and answered even if the reply is negative. All such information is indispensable if one should want to draw up an ethnographic atlas of Europe, or what is perhaps even more important, to encourage international teams of European specialists to investigate such matters as shepherding or fishing, for instance. Representatives of European ethnology should exchange opinions about selection of topics for investigation. The results that could be expected from well-organized international cooperation are exemplified by the success of the International Geophysical Year.—WOLFGANG JACOBET, Institute for German Ethnology, Platanenstr. 9 I, Berlin-Niederschönhausen, Germany.

For Sale

► Malinowski Reprints

The CENTRO DE INVESTIGACIONES ANTROPOLOGICAS DE MEXICO, Apartado Postal 2242, Mexico D.F., Mexico, announces that reprints of the following studies by Bronislaw Malinowski are available in limited numbers. Remittances should accompany orders, and reprints should be ordered according to the number under which they are listed here:

1. The Dynamics of Contemporary Diffusion. (*International Congress of Anthropological and Ethnological Sciences, Copenhagen, 1934.*) 4 pp. U.S. \$0.10, Mex. P.1.25.
2. The Anthropological Study of Sex. (in *Verhandlungen des I. Int. Kongr. für Sexualforschung, Berlin, 1926*; ed. Berlin 1928.) 17 pp. U.S. \$1.50, Mex. P.18.75.
3. Culture as a Determinant of Behavior. (Paper delivered at the Harvard Tercentenary Conference of Arts and Sciences. Harvard University Press, Cambridge, 1936.) 36 pp. U.S. \$1.00, Mex. P.12.50.
4. Practical Anthropology (no date). 17 pp. U.S. \$0.50, Mex. P.6.25.
5. Anthropology as the Basis of Social Science. (1936.) 53 pp. U.S. \$1.00, Mex. P.12.50.
6. Prenuptial Intercourse between the Sexes in the Trobriand Islands, N.W. Melanesia. (in *The Psychoanalytic Review*, vol. 14, no. 1, January 1927.) 16 pp. U.S. \$1.50, Mex. P.18.75.
7. The Scientific Basis of Applied Anthropology. (in *Reale Accademia D'Italia* 16, Rome, 1938.) 24 pp. U.S. \$0.50, Mex. P.6.25.
8. Modern Anthropology and European Rule in Africa. (in *Reale Accademia d'Italia* 16, Rome, 1938.) 24 pp. U.S. \$0.50, Mex. P.6.25.
9. An Introduction to Law and Order in Polynesia (London and New York). 56 pp. U.S. \$1.00, Mex. P.12.50.
10. Foreword to *Coming into Being among the Australian Aborigines. A Study of the Native Tribes of Australia*, by M. F. Ashley-Montagu (London, 1937). 30 pp. U.S. \$0.50, Mex. P.6.25.
11. Native Education and Culture Contact. (in *The International Review of Missions* 1936.) 36 pp. U.S. \$0.75, Mex. P.10.00.
12. The Present State of Studies in Culture Contact. (in *Africa*, vol. 11, no. 1, London, 1938.) 44 pp. U.S. \$0.50, Mex. P.6.25.
13. Introductory essay on "The Anthropology of Changing African Cultures." (in *Methods of Study of Culture Contact in Africa*, Memorandum 15 of the International Institute of African Languages and Cultures, London.) 32 pp. U.S. \$0.50, Mex. P.6.25.
14. "Culture." (in *Encyclopedia of the Social Sciences*.) 23 pp. U.S. \$0.50, Mex. P.6.25.
15. Forschungen in einer mütterlichen Gemeinschaft (auf den Trobriand-Inseln, östlich von Neu-Guinea, Südsee). (in *Zeitschrift für Völkerpsychologie und Soziologie*, Heft 1, Jahrgang 1, Leipzig, 1925.) 9 pp. U.S. \$0.50, Mex. P.6.25.

► Source materials for anthropologists, including reprints of important volumes and facsimile reproductions of codices and documents, are issued by the AKADEMISCHE DRUCK-UND VERLAGSANSTALT, Graz, Austria. Recent publications include the Mixtec *Codex Vindobonensis Mexicanus I* and *Codex Egerton* 2895; *Cartas de Relación de la Conquista de la Nueva España*, a collection of documents from the years 1519–1527; Leo Frobenius' *Die Afrikanischen Felsbilder*; *Monumenta Linguae Canariae*, by Dominik Josef Wölfel; *Grammatik der Athiopischen*

Sprache, by August Dillman; *Sanskrit-Wörterbuch*, by Otto Nikolaus von Böhtlingk; *Gesammelte Abhandlungen zur amerikanischen Sprach- und Altertumskunde*, by Eduard Seler; and *Lexicon Persico-Latinum Etymologicum*, by Johann August Vullers. Reprint editions of Benozoni's *Historia del Mondo Nuovo* and Ulrich Schmidel's *Wahrhaftige Historia* will be issued in the near future. A reprint of the original editions of Peter Martyr's *Opus Epistolarum*, *Legatio Babylonica*, and *Decades* is available at a special subscription price of ö S 980, DM 163.30, or U.S. \$37.70 until June 30, 1961. Prospectuses and further information may be obtained from Dr. H. Biedermann, Akademische Druck- u. Verlagsanstalt, Auerspergasse 12, Graz, Austria.

Offered

A limited printing of the papers delivered at the Symposium on Salvage Archeology held at the Section H meetings of the American Association for the Advancement of Science in New York, December 29, 1960, is now available for free distribution. Included are "A World View of Archeological Salvage and a Summation of the Aswan Dam International Co-operative Effort," by J. O. Brew; "Description and Evaluation of the Use of the Proton Magnetometer in the Field" (first U.S. exposition), by R. E. Linington; "Salvage in Great Britain and Colonial Sites in America," by I. Noël Hume; "Archeological Conservation in the People's Republic of China," by Frank Ridley; "Archeological Salvage in Canada," by Richard Forbis; "Archeological Salvage in Mexico," by Luis Avelleyra A. de Anda; and articles on river-basin and highway salvage in the U.S. by W. W. Wasley, Alexander Lindsay, John Corbett, and W. A. Ritchie. Copies of the complete symposium, and its conclusions, may be obtained by writing to the Chairman, JOHN L. COTTER, Regional Archeologist, Region Five, National Park Service, 143 South Third Street, Philadelphia 6, Pennsylvania, U.S.A.

Wanted

► Copies of the following publications: Lillard, J. B., and W. K. Purves, *The Archaeology of the Deer Creek Area, Sacramento County, California*, Sacramento Junior College Department of Anthropology Bulletin 1, 1936; and Lillard, J. B., R. F. Heizer, and F. Fenenga, *An Introduction to the Archaeology of Central California*, Sacramento Junior College Department of

Anthropology Bulletin 2, 1939. Extremely important to the archeology of central California, these publications are virtually lacking here.—DONALD P. JEWELL, American River Junior College, 4700 College Oak Drive, Sacramento 21, Calif., U.S.A.

► . . . (1) A copy (e.g. microfilm) of McCulloch, W., (1859) "Account of the valley of Munnipore and of the hill tribes . . ." Selections from the Records of the Government of India (Foreign Department) 27, Appendix. Calcutta.

(2) A copy of (Anonymous) Champak bijoy (in Bengali), published in Agartala (Tripura); no date.

(3) Bibliographical data on a paper on Chakma Coins, recently written by a Bengali scholar (in Bengali?); and if possible, a copy of the paper.

(4) Information about the year and place of publication of: Brown, J. F., General Report of the Tipperah District; and Rickett —, Wild Tribes of the Chittagong Frontier.

Kindly notify DR. L. G. LÖFFLER, Institut für Völkerkunde, University of Mainz, Mainz, Germany.

Institutions

► International Institute for Research in New Guinea

Formation of a special institute to direct and finance urgent anthropological field research in New Guinea was recommended by the Sixth International Congress of Anthropological and Ethnological Sciences. The proposal was originated by Jan van Baal, Department of Cultural and Physical Anthropology, Royal Tropical Institute, Amsterdam, Netherlands. Reviewing the conditions that make New Guinea an area of special interest to anthropologists, Dr. van Baal noted that the approaching extension of administrative control to every part of New Guinea, and the rapid cultural change that would follow, allowed, at most, fifteen years for research on disappearing ways of life in the area. The plan he offered called for the establishment of an institute that would employ 40 field-workers for a period of eight years each. The initial steps proposed were (1) a recommendation emphasizing the urgency of thorough field research in New Guinea, and (2) appointment of an international committee authorized to take necessary measures toward establishing such an institute.

The recommendation was adopted by the Committee on Urgent Anthropological and Ethnological Research, and submitted by Robert Heine-Geldern, to the International Union of Anthropological and Ethnological Sciences,

which accepted it without discussion.

Later, at an informal meeting, delegates from Great Britain (Raymond Firth, Anthony Forge), the U.S.A. (Joseph B. Casagrande, Albert C. Spaulding, Alex Spoehr, Sol Tax), and the Netherlands (H. Th. Fischer, J. Pouwer, and J. van Baal) agreed that as the first phase of establishing the institute anthropologists and national scientific institutes in the six countries (U.S., France, New Zealand, Australia, Netherlands, U.K.) co-operating in the South Pacific Commission should be approached.

Once the co-operation of national research institutes and anthropologists interested in New Guinea has been secured, the next step will be a meeting of 15-20 delegates to discuss and lay down the final plan and program. By now the phase of contacting a number of leading anthropologists interested in the area has been completed. It is hoped that the Pacific Science Congress, which will meet in Hawaii August 21-September 2, 1961, will provide an opportunity for achieving a more definite form of international co-operation.

► A GREEK CENTER OF SOCIAL RESEARCH has been established to plan social research and encourage the social sciences in Greece. The Center is governed by a Council of Professors and representatives of government ministries; its President is Professor Stratis Adreadis, and Professor George Pantazis is Vice-President. Professor John Peristiany, the present UNESCO Expert and Professor of Sociology, is directing teaching and research at the Center. A sociological library donated by UNESCO, to which will be added literature contributed by Greek sources, is housed in quarters provided by the Government of Greece, which include space for lectures and seminars. Social scientists engaged in research in Greece are urged to use the Center as a clearing house for sociological information. Those who plan to carry out such research are asked to write to the Secretary, The Social Science Center, 79 Skoufa Street, Athens, Greece.

► THE RESEARCH INSTITUTE FOR THE STUDY OF MAN, New York, U.S.A., is an educational and scientific organization concerned with problems in the behavioral sciences. Founded in 1955, the Institute initiates and sponsors original research by independent scholars; establishes graduate student training programs; plans and sponsors conferences; publishes research findings; offers consultation in research planning and methodology; and collaborates with other organizations in programs of applied social science.

Caribbean studies have been a major concern of the Institute, which provided funds to establish the Research and Training Program for the Study of Man in the Tropics (RTPSMT) at Columbia University, affiliated with its Department of Anthropology. Organized to meet the needs of social science research and field training in peasant and plantation subcultures in developing areas, the Program for the Study of Man in the Tropics focused on problems of social, economic, and cultural adaptation. To provide a basis for cross-cultural analysis and establish models for research in similar societies, an initial 3-year project was undertaken in the Caribbean, under supervision of a scientific advisory committee. Besides intensive field studies, the project included seminars at Columbia University, and four summer field laboratories in the Caribbean area. The student training program led to more intensive independent studies in the Caribbean, wholly or partly subsidized by RTPSMT. Besides studies of peasant and plantation subcultures, research has been devoted to social psychology, personality in culture, linguistic studies (of English and French Creole), and cultural factors in soil erosion. The field data have been made available for comparative studies, and a cross-cultural index, based on the categories of the Human Relations Area Files, has been organized.

A program of scholarly exchange was established with Caribbean institutes (particularly the Institute of Social and Economic Research, University College of the West Indies, Jamaica) and with individual scholars, providing exchange visits, seminars, and grants for travel to conferences.

A conference program was established to increase collaboration in research and facilitate exchange of knowledge among scholars who had been working at distances from each other. This program included the First Inter-American Conference on Caribbean Research, held December 1956 in New York; the Seminar on Plantation Systems in the New World (ca July 1960, page 337), held in Puerto Rico in November 1957, in cooperation with the Pan American Union and the Commonwealth of Puerto Rico; and the Conference on Social and Cultural Pluralism in the Caribbean, in May 1959 at New York, under the joint auspices of the Institute and the New York Academy of Sciences. A conference dealing with the political sociology of developing nations will be held in December 1961 in Jamaica, in cooperation with the Institute of Social and Economic Research, University College of the West Indies.

Besides its continuing interest in the Caribbean area, the Institute has brought together interdisciplinary research findings and proposals in the fields of medicine and social science. In co-operation with the New York Academy of Sciences, it organized a 3-day Conference on Culture, Society, and Health, held in New York, June 1-3, 1960, of which the papers and proceedings have been published as a monograph of the New York Academy of Sciences.

Current research activities of RISM include a census of mental hospitals in the Caribbean; cross-cultural pilot studies of the attitudes of medical students; a cultural-demographic study of fertility and mating patterns in Trinidad; a survey of the aspirations and attitudes of Trinidadian youth; a study

of linguistic barriers to communication among the Navaho; and a study of the culture of youth in the U.S.A.

A progress report covering the first five years of the Institute's existence is available on request from: The Research Institute for the Study of Man, 162 East 78th Street, New York 21, N.Y., U.S.A.

► A SEMINAR ON MAYA CULTURE has been established as part of the Faculty of Philosophy and Letters at the UNIVERSIDAD NACIONAL AUTONOMA DE MÉXICO to further anthropological investigations in the field of Mayan studies. Alberto Ruz Lhuillier is Director of the Seminar, whose honorary advisers are Alfred V. Kidder, Herbert J. Spinden, Alfonso Caso and Daniel R. de la Borbolla. J. Eric Thompson serves simul-

taneously as advisor and permanent investigator.

Permanent investigators are Calixta Guiteras Holmes, Alfonso Villa Rojas, Alfredo Barrera Vásquez and César Lizardi Ramos. All the investigators who have worked on or are now active in any discipline of anthropology dealing with the Mayan area are considered occasional collaborators.

The Seminar's activities include field research and library studies; publication of monographs and an annual review; republication of historical sources; and the holding of courses, seminars and lectures.

About one hundred Maya specialists have been asked to collaborate with the Seminar.

Further information will be sent by the Seminar to those who are interested.

Journal Contents

In this section we list the main articles in widely circulated journals, most of the contributors to which are Associates in CURRENT ANTHROPOLOGY. The selection of journals which follows reflects the material on hand at the time of publication, and will be expanded in future issues. Editors of these and similar publications are requested to send tables of contents even before forthcoming issues are completed. Sources for further information, when known, are indicated in the footnotes.

Acta Linguistica¹

Volume 10, Nos. 1-2, 1960

- B. A. SEREBRENNIKOV: Über den Charakter des Verbal-systems des Alt-Kasantatarschen.
 R. HUSSON: Le fonctionnement du larynx comparativement dans la parole et dans le chant.
 IRENE N. SEBESTYÉN: Zur Fragen der determinierenden Deklination im Jurak-samojedischen.
 P. Hajdú: The Form of the Object in Forest Yurak.
 F. KOVÁCS: Some Remarks on Uralic Numerals.
 B. KOROMPAY: Die finnisch-ugrische Ethnologie.
 LASZLO VÉRTES: Beiträge zur Frage des finnisch-ugrischen bezeichneten Akkusativ objekts.

America Indígena²

Volume 20, No. 1, January 1960

- DARCY RIBERO and others: Un Concepto sobre Integración Social.
 LLOYD LENTON: Indians of Canada.
 NATALICIO GONZÁLEZ: Calderón de la Barca y el Mundo Indígena.
 La Misión Andina en el Ecuador, trabajo preparado por el Instituto Ecuatoriano de Antropología y Geografía.
 ÁLVARO JARA: La Estructura Económica en Chile durante el siglo XVI.
 JUAN FRIEDE: Los Indios y la Historia.

Volume 20, No. 3, April 1960

- JUAN COMAS: Modalidades del Abandono en las Colectividades Indígenas.
 ROBERTO CARDOSO DE OLIVEIRA: The Role of Indian Posts in the Process of Assimilation—two case studies.
 ANÍBAL BUTRÓN: La Investigación y el Mejoramiento de las Condiciones de Vida.
 MARIA SULIA POURCHET: Subnutrição da Criança Indígena.
 LUIS LEAL: La Licantropía entre los Antiguos Mexicanos.
 MANNING NASH: Witchcraft as Social Process in a Tzeltal Community.
 LÁZARO FLURY: Supervivencia de Ritos Indígenas en el Noroeste Argentino.
 LEÓN CADOGAN: En torno a la Aculturación de los Mbyá-Guaraní del Guairá.

Volume 20, No. 3, July 1960

- ANÍBAL BUTRÓN: Problemas Económico-Sociales de la Educación en América Latina.
 RICHARD B. SCOTT: English Language Skills of the Mescalero Apache Indians.
 ALEJANDRO LIPSCHUTZ: La "Comunidad" y el Problema indígena en Chile.
 ROBERTO WILLIAMS GARCÍA: Una novela de Rectificación Antropológica.
 CATALINA GÁRATE DE GARCÍA: Los Trastornos Emocionales como causa de la Enfermedad en Tehuantepec.
 JACOB LOEWEN: A Choco Miraculous Escape Tale.
 JAMES RORTY: Hay Discriminación en México? La Experiencia Indigenista de Chiapas.

Volume 20, No. 4, October 1960

- JUAN COMAS: La vida y la Obra de Manuel Gamio (1883-1960).
 JOHN COLLIER: Dr. Manuel Gamio and the Instituto Indigenista Interamericano.
 IGNACIO MARQUINA: La Obra de Manuel Gamio.
 ÁNGEL MARÍA GARIBAY K.: La Obra de Gamio en Teotihuacán.
 EUSEBIO DÁVALOS HURTADO: Oración Fúnebre.
 MARGARITA GAMIO DE ALBA: El Dr. Manuel Gamio y el Proyecto de la Mujer Indígena.
 MIGUEL LEÓN PORTILLAS: Algunas Ideas Fundamentales del Dr. Gamio.
 DANIEL MORENO: El Pensamiento Socio-Económico de don Manuel Gamio.

Volume 21, No. 1, January 1961

- WILLARD W. BEATTY: History of Navajo Education.
 MIGUEL LEÓN-PORTILLA: El Legado Intelectual y Literario de las Culturas Indígenas Americanas.
 JOSÉ R. SABOGAL WIESSE: La Comunidad Andina de Pucará.
 WENDELL H. OSWALT: Guiding Culture Change among Alaskan Eskimos (First Part).

American Anthropologist³

Volume 62, No. 1, February 1960

- GEORGE W. STOCKING, JR.: Franz Boas and the Founding of the American Anthropological Association.
 EVON Z. VOGT: On the Concepts of Structure and Process in Cultural Anthropology.
 ROBERT F. GRAY: Sonjo Bride-Price and the Question of African "Wife Purchase."
 ANTHONY F. C. WALLACE AND JOHN ATKINS: The Meaning of Kinship Terms.
 HAROLD HICKERSON: The Feast of the Dead Among the Seventeenth Century Algonkians of the Upper Great Lakes.
 J. VAN BAAL: Erting Acculturation.
 RICHARD SLOBODIN: Some Social Functions of Kutchin Anxiety.
 RAYMOND A. DART: The Bone Tool-Manufacturing Ability of Australopithecus Promethus.

Volume 62, No. 2, April 1960

- FELIX M. KEESING: The International Organization of Anthropology.
 MARGARET LANTIS: Vernacular Culture.
 STEVEN POLGAR: Biculturalization of Mesquakie Teenage Boys.
 RODNEY NEEDHAM: Chawte Social Structure.
 JOHN W. BENNETT AND LEO A. DESPRES: Kinship and Instrumental Activities: A Theoretical Inquiry.
 JOHN C. MESSENGER, JR.: Reinterpretations of Christian and Indigenous Belief in a Nigerian Nativist Church.
 JUNE HELM MACNEISH: Kin Terms of Arctic Drainage Déné: Hare, Slavey, Chipewyan.
 WAYNE SUTTLES: Affinal Ties, Subsistence, and Prestige among the Coast Salish.
 TAKAO SOFUE: Japanese Studies by American Anthropologists: Review and Evaluation.
 NORMAN A. MCQUOWN: American Indian and General Linguistics.

Volume 62, No. 3, June 1960

- MICHAEL D. COE: Archeological Linkages with North and South America at La Victoria, Guatemala.
 BERNARD J. SIEGEL AND ALAN R. BEALS: Pervasive Factionalism.
 MIGUEL LAYRISSE, ZULAY LAYRISSE, and JOHANNES WILBERT: Blood Group Antigen Tests of the Yupa Indians of Venezuela.
 LOUIS C. FARON: The Formation of two Indigenous Communities in Coastal Peru.
 MARY H. LYSTAD: Traditional Values of Ghanaian Children.
 FRANK J. MAHONY: The Innovation of a Savings System in Truk.
 GEORGE DALTON: A Note of Clarification on Economic Surplus.
 C. F. VOEGELIN: Pregnancy Couvade Attested by Term and Text in Hopi.
 TERTIUS CHANDLER: Duplicate Inventions?

Volume 62, No. 4, August 1960

- MELVILLE J. HERSKOVITS: The Ahistorical Approach to Afroamerican Studies: A Critique.
 WESLEY R. HURT, JR.: The Cultural Complexes from the Lagoa Santa Region, Brazil.
 ALEXANDER SPOEHR: Port Town and Hinterland in the Pacific Islands.
 ROBERT F. MAHER: Social Structure and Cultural Change in Papua.
 SEYMOUR PARKER: The Witiko Psychosis in the Context of Ojibwa Personality.
 H. M. BLALOCK, JR.: Correlational Analysis and Causal Inferences.
 BERNARD GALLIN: Matrilateral and Affinal Relationships of a Taiwanese Village.
 ALFRED G. SMITH and JOHN P. KENNEDY: The Extension of Incest Taboos in the Woleai, Micronesia.
 E. ADAMSON HOEBEL: William Robertson: An 18th Century Anthropologist-Historian.
 JOHN T. and PATRICIA J. HITCHCOCK: Some Considerations for the Prospective Cinematographer.

Volume 62, No. 5, October 1960

- ELMAN R. SERVICE: Kinship Terminology and Evolution.
 ROBERT C. SUGGS: Historical Traditions and Archeology in Polynesia.
 GERALD D. BERREMAN: Cultural Variability and Drift in the Himalayan Hills.
 ARTHUR J. RUBEL: Concepts of Disease in Mexican-American Culture.
 HARUMI BEFU and CHESTER S. CHARD: Pre-ceramic Cultures in Japan.
 J. A. BARNES: Marriage and Residential Continuity.

ALEXANDER VUCINICH: Soviet Ethnographic Studies of Cultural Change.

Volume 62, No. 6, December 1960

FRED W. VOGET: Man and Culture: An Essay in Changing Anthropological Interpretation.

OCTAVIO IGNACIO ROMANO V.: Donship in a Mexican-American Community in Texas.

IVAN A. LOPATIN: Origin of the Native American Steam Bath.

G. I. C. INGRAM: Displacement Activity in Human Behavior.

ROBERT PAINE: Emergence of the Village as a Social Unit in a Coast Lappish Fjord.

OZZIE G. SIMMONS: Ambivalence and the Learning of Drinking Behavior in a Peruvian Community.

NORMAN A. CHANCE: Culture Change and Integration: An Eskimo Example.

Volume 63, No. 1, February 1961

GEORGE DALTON: Economic Theory and Primitive Society.

WALTER GOLDSCHMIDT and ROBERT B. EDGERTON: A Picture Technique for the Study of Values.

EUGENE GILES and HERMANN K. BLEIBTREU: Cranial Evidence in Archeological Reconstruction: A Trial of Multivariate Techniques for the Southwest.

HUGO G. NUTINI: Clan Organization in a Nahuatl-Speaking Village of the State of Tlaxcala, Mexico.

J. L. FISCHER: Art Styles as Cultural Cognitive Maps.

I. M. LEWIS: Force and Fission in Northern Somali Lineage Structure.

CHARLES O. FRAKE: The Diagnosis of Disease among the Subanon of Mindanao.

American Antiquity⁴

Volume 25, No. 4, April 1960

TATIANA PROSKOURIAKOFF: Historical Implications of a Pattern of Dates at Piedras Negras, Guatemala.

IRVING FRIEDMAN and ROBERT L. SMITH: A New Dating Method Using Obsidian.

Part I, The Development of the Method.

CLIFFORD EVANS and BETTY J. MEGGERS: A New Dating Method Using Obsidian.

Part II, An Archaeological Evaluation of the Method.

ELAINE BLUHM: Mogollon Settlement Patterns in Pine Lawn Valley, New Mexico.

J. CHARLES KELLEY and HOWARD D. WINTERS: A Revision of the Archaeological Sequence in Sinaloa, Mexico.

ROY L. CARLSON: Chronology and Culture Change in the San Juan Islands, Washington.

Volume 26, No. 2, October 1960

LINTON SATTERTHWAIT and ELIZABETH K. RALPH: New Radiocarbon Dates and the Maya Correlation Problem.

ALAN P. OLSON: The Dry Prong Site, East Central Arizona.

GEORGE M. FOSTER: Archaeological Implications of the Modern Pottery of Acatlán, Puebla, Mexico.

ROBERT F. HILZER: Agriculture and the Theocratic State in Lowland Southeastern Mexico.

HERBERT W. DICK and BERT MOUNTAIN: The Claypool Site: A Cody Complex Site in Northeastern Colorado.

HAROLD E. MALDE: Geological Age of the Claypool Site, Northeastern Colorado.

WILLIAM W. WASLEY: A Hohokam Platform Mound at the Gatlin Site, Gila Bend, Arizona.

Anthropological Linguistics⁵

Volume 2, No. 1, January 1960

G. N. O'GRADY: New Concepts in Nyanu-

mada: Some Data on Linguistic Acculturation.

RALPH MORGAN, JR.: The Lexicon of Saint Martin Creole.

Volume 2, No. 2, February 1960

FLORENCE R. KLUCKHOHN: A Method for Eliciting Value Orientations.

GEORGE L. TRAGER: Taos III: Paralanguage.

STANLEY DIAMOND: Anaguta Cosmography: The Linguistic and Behavioral Implications.

MORRIS SWADESH: On the Unit of Translation.

ZDENĚK SALZMANN: Cultures, Languages, and Translations.

C. F. and F. M. VOEGELIN: Selection in Hopi Ethics, Linguistics, and Translation.

Volume 2, No. 3, March 1960

C. DOUGLAS ELLIS: A Note on Okima-hka-n.

WILLIAM M. JONES: Origin of the Place-Name Taos.

GEORGE L. TRAGER: The Name of Taos, New Mexico.

DAVID D. THOMAS: Basic Vocabulary in Some Mon-Khmer Languages.

DORITA LOCHAK: Basque Phonemics.

Volume 2, No. 4, April 1960

A. L. KROEBER: Powell and Henshaw: An Episode in the History of Ethnolinguistics.

WILLIAM SEIDEN: Chamorro Phonemes.

Volume 2, No. 5, May 1960

AUGUST C. MAHR: Anatomical Terminology of the Eighteenth-Century Delaware Indians: A Study in Semantics.

Volume 2, No. 6, June 1960

DONALD C. SIMMONS: Tonal Rhyme in Efik Poetry.

HARJEET SINGH GILL: Panjabi Tonemics.

ROGER W. WESCOTT: The Metalinguistics of Bini: A West African Language.

JESSAMINE UPSON: A Preliminary Structure of Chatino.

Volume 2, No. 7, October 1960

WILLIAM W. ELMENDORF and WAYNE SUTTLES: Pattern and Change in Halkomelem Salish Dialects.

JOHN R. KRUEGER: Miscellanea Salica I: A Flathead Supplement to Vogt's Salishan Studies.

ZDENĚK SALZMANN: Two Brief Contributions Toward Arapaho Linguistic History.

Volume 2, No. 8, November 1960

ROBERT H. LOWIE: A Few Assiniboiné Texts.

Volume 2, No. 9, December 1960

MARVIN K. MAYERS: The Phonemics of Pocomchi.

Volume 3, No. 1, January 1961

THEODORE SCHWARTZ and MARGARET MEAD: Micro- and Macro-Cultural Models for Cultural Evolution.

RALPH L. BEALS: Community Typologies in Latin America.

GEORGE L. TRAGER: The Typology of Paralanguage.

DELL H. HYMES: On Typology of Cognitive Styles in Language.

C. F. and F. M. VOEGELIN: Typological Classification of Systems with Included, Excluded and Self-Sufficient Alphabets.

Volume 3, No. 2, February 1961

KITTY PRIDE: Numerals in Chatino.

JOHN R. KRUEGER: Miscellanea Salica II: Some Kinship Terms of the Flathead Salish.

FRANCIS P. CONANT: Jarawa Kin Systems of Reference and Address: A Componential Comparison.

Anthropological Quarterly

Volume 33, No. 1, January 1960

JULIAN PITT-RIVERS: Social Class in a French Village.

MICHAEL KENNY: Patterns of Patronage in Spain.

LEONARD W. MOSS and STEPHEN C. CAPANNARI: Patterns of Kinship, Comparaggio and Community in a South Italian Village.

DONALD S. PITKIN: Marital Property Considerations Among Peasants: An Italian Example.

ROBERT F. SPENCER: Aspects of Turkish Kinship and Social Structure.

A. P. STIRLING: A Death and a Youth Club: Feuding in a Turkish Village.

LAWRENCE KRADER: The Transition from Serf to Peasant in Eastern Europe.

Volume 33, No. 2, April 1960

MORRIS E. OPLER: Recent Changes in Family Structure in an Indian Village.

ROBERT T. ANDERSON and GALLATIN ANDERSON: Changing Social Stratification in a Danish Village.

FRADA NAROLL: Child Training among Tyrolean Peasants.

ANTONIO SANTA CRUZ: Acquiring Status in Guajiro Society.

Volume 33, No. 3, July 1960

FRANKLIN EWING, S. J.: Birth Customs of the Tawsug, Compared with Those of Other Philippine Groups.

BRIAN M. DU TOIT: Some Aspects of the Soul-Concept Among the Bantu-Speaking Nguni-Tribes of South Africa.

HUGH H. SMYTHE: Urbanization in Nigeria.

SIMON D. MESSING: A Modern Ethiopian Play—Selfstudy in Culture Change.

HUGO O. ENGELMANN: The Active Bias of Ethnography and the History of Society.

Volume 33, No. 4, October 1960 (Caribbean Issue)

SIDNEY M. GREENFIELD: Land Tenure and Transmission in Rural Barbados.

MICHAEL M. HOROWITZ: A Typology of Rural Community Forms in the Caribbean.

MORRIS FREILICH: Cultural Models and Land Holdings.

Volume 34, No. 1, January 1961

FRANK CANCIAN: The Southern Italian Peasant: World View and Political Behavior.

ROBERT K. BURNS, JR.: The Ecological Basis of French Alpine Peasant Communities in the Dauphine.

Anthropological Reports⁶

No. 28, February 1960

TAKESHI TERAOKA: On the Sole Prints of the Inhabitants of Oki Island, Shimane Prefecture.

SHIGERU KATAGIRI: Roentgenogrammetry of the Tissue Components in the Leg of Japanese Boys and Girls.

SHIN MIYAZAWA: Roentgenogrammetry of the Tissue Components in the Upper Extremities of Japanese Boys and Girls.

KAKUTARO KITAMI, SHOJI TAKAHASHI, SUMI SAKAI and SHOICHI INOUE: On Growth and Aging in Metrical Characters of Head and Face (Supplements for the Anthropological Studies of Sado Islanders).

RIKISO SEO, MASANORI KIMURA and MASANORI ASHIKAGA: Somatommetrical Studies

on the Physical Changes with Aging in the Same Individuals (Supplementary Report).

No. 29, May 1960

- NOBORU YAMASHITA: Studies on Palmar and Plantar Patterns of Twins.
MITSUO IWAMOTO and SHOJI TAKAHASHI: On Statistic Methods of Physical Asymmetric Degrees.
TOMOYO OYAKE and MITSUO IWAMOTO: On the Sagittal Curvature of the Vertebral Column of Operated Pulmonary Tuberculosis Patients.
TOSHIO FUJIMURA: Study on Toe Prints in Japanese.

No. 30, July 1960

- NOBORU YAMASHITA: On the Aberrant Formation of Epidermal Ridges of Fingers and Toes in Feeble-minded Children.
TOSHIO FUJIMURA: On Toe Prints of Korean Males and Formosan Males.
TOSHIO FUJIMURA: Supplemental Study on Toe-Prints in Japanese.
AKIMICHI SASAKURA: An Anthropological Study on the Toe and Finger Prints of Inhabitants in the Shikoku District.

No. 31, September 1960

- AKIMICHI SASAKURA: On the Toe and Finger Prints of the Inhabitants in the Shinshu District.
TADAO HASHIMURA: A Study on Toe and Finger Prints of the Inhabitants in Kyushu District.
YUKITAKA TERAKADO, AKIMICHI SASAKURA and TOSHIMITSU HASHIMURA: A Supplement to the Anthropological Measurement of Inhabitants of Oki Islands. On the Inhabitants of Dōgo (North Island).
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- PAUL DARASSE and SIMONE GUFFROY: Le Magdalénien supérieur de l'abri de Fontalès, près Saint-Antonin (Tarn-et-Garonne).
G. KURT: Les restes humains würmiens du gisement de Shanidar, Nord-Est Irak.
J. G. HENROTTE: Quelques données biométriques sur l'indien de Madras et son adaptation au climat tropical.
PIERRE DUCOS: Note sur le cheval néolithique en France.
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- GÉRARD BAILLOU: Les peintures rupestres archaïques de l'Ennedi (Tchad).
A. G. DE WILDE: Deux crânes fossiles trouvés dans la Meuse, près de Lith, en Hollande.
NICOLE HEINTZ: Contribution à l'étude comparative de la croissance du nez chez les Noirs et les Blancs de 0 à 80 ans.

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- FRITZ LENZ: Nun doch wieder "Wirbel-säulenmethode"?
HELMUT BAITSCH: Zur Kenntnis der Haptoglobulin-Typen einiger Cercopithecinae.
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- RAYMOND A. DART: The status of Gigantopithecus.
A. REMANE: Die Stellung von Gigantopithecus.

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G. H. R. VON KOENIGSWALD: Bemerkungen zum Skelettkult: Beobachtungen auf Java und den Philippinen.

SOPHIE EHRHARDT: Schlagspuren, Brüche und Sprünge an den Skeletten von Langhnaj im nördlichen Gujarat, Vorderindien.

HANS W. JÜRGENS: Korrekturen für die Längenmessung am Unterschenkel.

R. SIEG: Zur Planung und Durchführung anthropologischer Reihen Untersuchungen an Kleinkindern.

M. WENINGER: Ein seltener Fall von Alterswandel an den Weichteilen der Augengegend.

IRMGARD TILLNER und ERNA BÖSHAAR: Ein Beitrag zum Problem der Haarfarbenveränderung.

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DIETRICH WICHMANN: Zur Genetik der Augenwimpern.

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- E. A. WORMS: Tasmanian Mythological Terms.
K. JARITZ: Die Kulturreste der Kassiten.
K. J. NARR: Hundert Jahre Neandertaler-Forschung.
J. SARMA: Puberty, Marriage and Childbirth among the Panggi and the Min-yong Abor Women.
J. ITTMAN: Orakelwesen im Kameruner Waldland.
H. AUFENANGER: Jugendweihe und Weltbild am mittleren Sepik.
TH. KAMAINDA: Le culte des morts chez les Balambo.
J. P. CRAZZOLARA: Notes on the Lango-Omitu and on the Labwoor and Nyakwai.
C. A. SCHMITZ: Die Problematik der Mythologeme "Hainuwele" und "Prometheus."

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- L. ADAM: The Abstract Art of the Aranda
K. BOUDA: Die Verwandtschaftsverhältnisse des Giljakischen.
V. ROSNER: Falconry as Followed by the Tribals in Biru.
J. MARINGER: Vorgeschichtliche Ritzzeichnungen in Stein und Fels Nordjapans.
M. GUSINDE and CH. SANO: Stone Circles in Northern Japan.
H. AUFENANGER: New Stone Implements from the Central Highlands of New Guinea.
M. VANOVERBERGH: Isneg Songs.
L. G. LÖFFLER: Khami/Khumi-Vokabulare.

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- H. BERGER: Bericht über sprachliche und volkskundliche Forschungen im Hunzatal.
F. H. MAYER: Character Portrayal in the Japanese Folk Tale.
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E. A. ZBINDEN: Nördliche und südliche Elemente im Kulturheroenmythus der Südhapasken.
B. MUKHERJEE: Cousin Marriage Among the Riang.
E. ZYHLARZ: Zum Typus der kaschitischen Sprache.
A. J. VAN WINDEKENS: Contacts linguistiques amou-tokhariens.

U. BERNER: Die Handhabung des Ackergerätes in ihrer Bedeutung für die Feststellung von Zusammenhängen.

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J. J. HONIGMANN: Education and Career Specialization in a West Pakistan Village of Renown.

TH. S. BARTHEL: Rezitationen von der Osterinsel.

R. MOHR: Einige Notizen über die Tangale von Biliri.

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Volume 12, No. 1, January 1960

- GORDON R. WILLEY, A. L. SMITH, W. R. BULLARD JR. y JOHN GRAHAM: Informe preliminar sobre Altar de Sacrificios.
PEDRO BOSCH-GIMPERA: La Prehistoria del Nuevo Mundo y Centro América.
CARLOS SAMAYOA CHINCHILLA: Notas sobre las causas que más influyeron en las derrotas de los ejércitos indígenas durante las guerras de conquista.
LUIS LUJÁN MUÑOZ: Nuevos aportaciones acerca de la introducción de la Imprenta en Guatemala.
HÉCTOR HUMBERTO SAMAYOA GUEVARA: Los gremios de artesanos en el Siglo XVIII.
ERNESTO CHINCHILLA AGUILAR: El Instituto Nacional de Guatemala.
MANUEL RUBIO SÁNCHEZ: Tenencia de la tierra durante la época independiente.

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No. 9, January 1960

- LEEDS, ANTHONY. The ideology of the Yafuto Indians in relation to socio-economic organization.
DAMIÁN DE ESCOBAR: Ensayo de penetración en la sabiduría y la filosofía indígenas.
HENRY OSBORN: Textos folklóricos Guaraos.

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Volume 13, No. 1, March 1960

- SARA A. IMMERWAHR: Mycenaean Trade and Colonization.
HELENE J. KANTOR: Ivory Carving in the Mycenaean Period.
EMMETT L. BENNETT, JR.: Anonymous Writers in Mycenaean Palaces.
MICHAEL JAMESON: Mycenaean Religion.
ALAN J. B. WACE: A Mycenaean Mystery.
J. WALTER GRAHAM: Mycenaean Architecture.
MABEL LANG: Picture Puzzles from Pylos.
M. S. F. HOOD: Schliemann's Mycenae Albums.
EMILY TOWNSEND VERMEULE: The Fall of the Mycenaean Empire.

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- S. K. LOTHROP: A Ceremonial Pottery Mask from Peru.
FRANÇOIS SALVIAT and NICOLE WEILL: The Sanctuary of Artemis in Thasos.
OSCAR BRONER: Isthmia: Campaign of 1959.
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ROBERT H. DYSON, JR.: Hasanlu and Early Iran.
JOHN L. CASKEY: Royal Shaft Graves at Letna.
MARVIN C. ROSS: Byzantine Bronze Peacock Lamps.

SAUL S. WEINBERG: Roman Twins: Basilicas at Corinth.

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GEORGE G. CAMERON: The Monument of King Darius at Bisitun.

MORTON SMITH: Monasteries and Their Manuscripts.

STANISLAW JASNOSZ: Provincial Roman Pottery in Poland.

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SPYRIDON N. MARINATOS: Helice: A Submerged Town of Classical Greece.

RICHARD N. MACNEISH: Archaeological Projects in Canada, 1959.

BRIAN A. SPARKES: Kottabos: An Athenian After-Dinner Game.

SABINE GOVA: Spina Rediviva.

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DOROTHY BURR THOMPSON: The House of Simon the Shoemaker.

RICHARD C. RUDOLPH: Bull Grappling in Early Chinese Reliefs.

R. ROSS HOLLOWAY: Tyndaris: Last Colony of the Sicilian Greeks.

KURT WILLVONSEDER: A Sculpture from Salzburg.

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DONALD N. WILBER: The Off-Beat in Egyptian Art.

FRANK C. HIBBEN: Prehispanic Paintings at Pottery Mound.

CHARLOTTE BLINDHEIM: New Light on Viking Trade in Norway.

EDITH PORADA: Orientalists Meet in Moscow.

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WILHELM G. SOLHEIM II: Introduction to Sa-huynh.

OLOV R. T. JANSE: Some notes on the Sa-huynh Complex.

LOUIS MALLERET: Quelques poteries de Sa-huynh dans leurs rapports avec divers sites du Sud-Est de l'Asie.

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B. A. V. PEACOCK: A Short Description of Malayan Prehistoric Pottery.

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G. DEBIEN: Destinées d'esclaves à la Martinique.

O. DU PUIGAUDEAU: L'architecture maure.

P. HUARD: Contribution à l'étude du cheval, du fer et du chameau au Sahara oriental.

P. HUARD et M. CHARPIN: Contribution à l'étude anthropologique des Teda du Tibesti.

B. KENNEDY: A necked and a lugged axe from Nigeria, and some preliminary observations on the "outils à gorge" family in Africa.

J. C. FROELICH et P. ALEXANDRE: Histoire traditionnelle des Kotokoli et des Bi-Tchambi du Nord-Togo.

R. P. MALGRAS: La condition sociale du paysan minyanka dans le cercle de San. M. PALAU MARTI: Conduites abusives permises en Afrique.

L. V. THOMAS: Essai d'analyse structurale appliquée à la cuisine diola.

Bulletins et Mémoires de la Société d'Anthropologie de Paris¹⁴

Volume 1, 11th Series, No. 1, January-March 1960

ROBERT-P. CHARLES: Le Peuplement de l'Europe Méditerranéenne pendant les III^e et II^e millénaires avant Jésus-Christ.

Volume 1, 11th Series, No. 2, April-June 1960

HENRI LEHMANN et PAULETTE MARQUER: Étude anthropologique des Indiens du groupe "Guambiano-Kokonuko" (région de Popayán, Colombie).

MM. R. KHERUMIAN et NIQUY: Note sur les groupes sanguins ABO des Brahmanes de Pondichéry.

A. RIGAUD, J. SOUTOUL et P. VAINGNEDEY: Les projections vertébrales et sternochondrales des grands orifices du diaphragme en fonction des types morphologiques thoraco-abdominaux et de l'indice thoraco-abdominal.

JEAN GAILLARD: Détermination sexuelle d'un os coxal.

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GEORGES BALANDIER: Structures sociales traditionnelles et changements économiques.

DENISE PAULME: Structures sociales traditionnelles en Afrique Noire.

PAUL MERCIER: Etude du mariage et enquête urbaine.

HENRI BRUNSCHWIG: Colonisation-Décolonisation. Essai sur le vocabulaire usuel de la politique coloniale.

MAX GLUCKMAN: Tribalism in modern British Central Africa.

DENISE PAULME: La société kissi: son organisation politique.

YVES PERSON: Soixante ans d'évolution en pays kissi.

JOSEPH TUBIANA: La mission du Centre National de la Recherche Scientifique aux confins du Tchad.

No. 2, October 1960

GILLES SAUTTER: Le plateau congolais de Mbé.

MARIE-JOSÉ TUBIANA: Un document inédit sur les sultans du Wadday.

GEORGES NIANGORAN-BOUAH: Le village abouté.

ERIC DE DAMPIERRE: Coton noir, café blanc (oubangui).

ETHEL M. ALBERTS: Une étude des valeurs en Urundi.

JEAN-CLAUDE PAUVERT: L'évolution politique des Ewé.

No. 3 (forthcoming)

G. DIETERLEN et G. CALAME-GRIAULE: L'alimentation dogon.

JACQUES LOMBARD: La vie politique dans une ancienne société de type féodal: Les Bariba du Dahomey.

ROBERT JAULIN: Note sur Marabé, le village mara.

GABRIEL SEVY: Le Wama des Ngbwaka de la Lobaye.

CLAUDE MEILLASSOUX: Essai d'interprétation du phénomène économique dans les sociétés traditionnelles d'autosubsistance.

KENNETH LITTLE: West African urbanization as a social process.

ARISTIDE R. ZOLBERG: Effets de la structure

d'un parti politique sur l'intégration nationale (Côte d'Ivoire).

IMMANUEL WALLERSTEIN: Ethnicity and national integration in West Africa (Côte d'Ivoire).

The Eastern Anthropologist¹⁶

Volume 13, No. 1, Sept.-Nov. 1960

HAROLD A. GOULD: The Peasant Village: Centrifugal or Centripetal?

HENRY H. PRESLER: Institutional Shifts in Religious Affiliation.

SURENDRA KUMAR NAVALAKHA: The Authority Structure among the Bhumi and Bhil: A study of Historical Causations.

PRABHAT CHANDRA: Rural Leadership in India.

Volume 13, No. 2, Dec. 1959-Feb. 1960 (Special Number)

GHAUS ANSARI: Muslim Caste in Uttar Pradesh (A study of Culture Contact).

Volume 13, No. 3, March-May 1960

D. G. KARVE: Some Sociological Implications of Planned Development.

KAMALA CHOWDHURY: Resistance to Change.

B. P. ADHIKARI: Construction of Socio-Econometric Models for Planning.

HIRENDRA K. RAKSHIT: Birth Order, Sibship Size and Sex Ratio.

P. K. BHOWMICK: Economic Life of the Lodhas.

RAMKRISHNA MUKHERJEE: Some Considerations on Social Research.

Ethnos¹⁷

Volume 25, No. 1-2, 1960

S. RYDÉN: Unknown Cook Collection Identified.

T. ODAYASHI: Die Amaterasu-Mythe im alten Japan.

V. DUSENBERRY: The Material Culture of the Assiniboine Indians.

H. V. WINNING: Figurines on Wheels from Mexico.

W. HABERLAND: Jointed Figurines from El Salvador.

L. PAULSON: Seelenvorstellungen und Totenglaube bei norddeutschen Völkern.

L. G. LÖFFLER: Patrilinear Lineation in Transition.

Etnografia Polska (Polish Ethnography)¹⁸

Volume 3, 1960 (Special volume in memory of the late Professor Kazimierz Moszyński (1887-1959))

MIECZYSLAW GLADYSZ (Cracow): Kazimierz Moszyński English (summary).

WŁODZIMIERZ ANTONIEWICZ (Warsaw): Horse and Cart Pictorial Rock Representations in North Africa and the Sahara (French summary).

BRANIMIR BRATANIĆ (Zagreb): on the Technical Construction of the Ancient Slavic Plough (English summary).

STANISLAW BRONICZ (Opole): Specimen of Old Plough Found in the Opole District (English summary).

SIGURD ERIKSON (Stockholm): The Change of the Place of Habitation and Old Primitive Food Relations in Sweden (Russian summary).

RYSZARD GANSINIEC (Cracow): Harvest Festivals of Classical Antiquity (English summary).

ZOFIA GANSINIEC (Cracow): The Woman—"Bialka" (English summary).

MILOVAN GAVAZZI (Zagreb): The Appearance of the West Pannonian Slavs in the Past (French Summary).

TADEUSZ STANISLAW GRABOWSKI (Cracow):

- Studies on the Folk-Art of Macedonia (French Summary).
- SULO HALTSONEN (Helsinki): Some interests and tasks of Finnish Ethnography (Russian Summary).
- WOJCIECH HEJNOSZ (Toruń): Forest-burning Cultivation in Early Medieval Poland (German summary).
- Jiří HORÁK (Praha): Ludwik Kuba and the Polish Folk Song (English summary).
- ERWIN KOSCHMIEDER (Munich): Sopila—a Folk Wind-instrument on Krku Island (German summary).
- JÓZEF KOSTRZEWSKI (Poznań): Fish-hooks with Spinnets (English summary).
- ANNA KUTRZEB-POJNAROWA (Warsaw): A New Textbook of Polish Ethnography (French summary).
- JÓZEF LIĞZA (Katowice): On Ethnographic Study of Musical Folklore (French summary).
- JÁN MJARTAN (Bratislava): Some Details of Popular Apiculture in the Horná Nitra Region (English summary).
- ADOLF NASZ (Wrocław): Harvest Implements in Poland (English summary).
- ROMAN REINFUSS (Cracow): Potters'-Ovens in Polish Folk Pottery (French summary).
- FRANCISZEK SŁAWSKI (Cracow): Two Items of Slavic Building Terminology (French summary).
- WITOLD TASZYCKI (Cracow): Slavic Metonymic Place Names (French summary).
- S. A. TOKARIEV (Moscow): Kinship Terminology—some Methodological Problems (English summary).
- STANISŁAW URBAŃCZYK (Cracow): An Old Polish Dictionary as Ethnographic Source (French summary).
- ANTONÍN VÁCLAVÍK (Brno): From Work to Popular Usages and Folk Art (English summary).
- CHRISTO WAKARELSKI (Sofia): Traces of the One-Field System of Cultivation in Bulgaria (English summary).
- LEOPOLD WĘGRZYNOWICZ (Cracow): Fedorki—the Christmas Puppet Show (French summary).
- MARIA ZNAMIEROWSKA-PRÜFFEROWA (Toruń): Material on the Ceremonial Life of the Karaims (French summary).

Homo¹⁰

Volume 11, Nos. 1-2, 1960

- F. KEITER: Über Erbeil und Selektion von Sozialverhaltensweisen.
- S. KOLLER: Der Zensus als Quelle Sozialanthropologischer Untersuchungen.
- H. WALTER: Sozialanthropologische Untersuchungen über die Herkunft der westfälischen Bevölkerung.
- H. W. JÜRGENS: Untersuchungen über die Herkunft von Kleiner Handwerkerfamilien.
- H. SCHADE: Sozial-anthropologische Ergebnisse einer Zensusuntersuchung (Schwalm).
- H. ARNOLD: Bevölkerungsbiologische Beobachtungen an Sippenwanderern.
- R. CLUQUET: On the differential population development of the Flemings and the Walloons and its influence on the Flemish-Walloons relations.
- K. V. MÜLLER: Der Stand der Forschung zur differentiellen Fortpflanzung und Begabungsauslese.
- I. SCHWIDETZKY: Bauern als Siebungsgruppen.
- K. GERHARDT: C. G. oder J. V. Catus?

Human Organization²⁰

Volume 19, No. 1, Spring 1960

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- WHYTE: Intercultural Communication: A Guide to Men of Action.
- JIRI KOLAJA: Workers' Participation in the Management of a Polish Textile Factory.
- DAVID RIESMAN, ROBERT J. POTTER and JEANNE WATSON: The Vanishing Host.
- NATHAN KEYFITZ: Western Perspectives and Asian Problems.
- P. O'NEAL, J. SCHAEFFER, J. BERGMANN, and L. N. ROBINS: A Psychiatric Evaluation of Adults Who Had Sexual Problems as Children.
- MILTON L. BARNETT: Kinship as a Factor Affecting Cantonese Economic Adaptation in the United States.
- BERNARD F. PIERCE: Anthropologists as Human Factors Engineers.

Volume 19, No. 3, Fall 1960 (Special Issue: Social Science in Action in Sub-Saharan Africa)

- LUCY MAIR: The Social Sciences in Africa South of the Sahara: The British Contribution.
- GEORGES BALANDIER: The French Tradition of African Research.
- JOSEPH NICAISE: Applied Anthropology in the Congo and Ruanda-Urundi.
- CHRISTOPHER BIRD: Soviet Ethnography: A Base for Applied Study of and Operations in Africa.
- PETER MARRIS: Slum Clearance and Family Life in Lagos.
- PETER C. W. GUTKIND: Congestion and Overcrowding: An African Urban Problem.
- WILLIAM A. HANCE: The Economic Location and Functions of Tropical African Cities.
- M. G. SMITH: Kagoro Political Development.
- ST. CLAIR DRAKE: Traditional Authority and Social Action in Former British West Africa.
- P. H. GULLIVER: Incentives in Labor Migration.
- HORACE MINER: Culture Change Under Pressure: A Hausa Case.

Volume 19, No. 4, Winter 1960-1961

- GEORGE M. FOSTER: Interpersonal Relations in Peasant Society.
- EVERETT E. HAGEN: The Entrepreneur as Rebel Against Traditional Society.
- LEWIS ANTHONY DEXTER: Where the Elephant Feels to Dance Among the Chickens.
- PAUL C. AGNEW and FRANCIS L. K. HSU: Introducing Change in a Mental Hospital.
- JAMES V. CLARK: Motivation in Work Groups: A Tentative View.
- WILLIS E. SIBLEY: Social Structure and Planned Change.
- MARION PEARSALL: A Model for the Analysis of Cross-Cultural Action Programs.

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Volume 26, No. 1, January 1960

- A. RICHARD DIEBOLD, JR.: Determining the Centers of Dispersal of Language Groups.
- WALLACE L. CHAFE: Seneca Morphology I: Introduction.
- CORNELIA MAK and ROBERT LONGACRE: Proto-Mixtec Phonology.
- WICK R. MILLER: Spanish Loanwords in Acome: II.
- GLADYS A. REICHARD: A Comparison of Five Salish Languages: VI.

Volume 26, No. 2, April 1960

- MORRIS SWADESH: The Oto-Manguean Hypothesis and Macro Mixtecan.
- SHIRLEY SILVER: Natick Consonants in Reference to Proto-Central Algonquian: I.

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HAROLD KEY: Stem Construction and Affixation of Sierra Nahuatl Verbs.

Volume 26, No. 3, July 1960

- A. L. KROEBER: On Typological Indices I: Ranking of Languages.
- JOSEPH H. GREENBERG: A Quantitative Approach to the Morphological Typology of Language.
- FRED W. HOUSEHOLDER, JR.: First Thoughts on Syntactic Indices.
- C. F. VOEGELIN, R. A. RAMANUJAN, and F. M. VOEGELIN: Typology of Density Ranges I: Introduction.
- BEN ELSON: Sieta Popoluca Morphology.
- WALLACE L. CHAFE: Seneca Morphology III: Expanded Pronominal Verbs.
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- TADEUSZ MILEWSKI: Similarities Between the Asiatic and American Indian Languages.
- HOWARD MACLAY and MARY D. SLEATOR: Responses to Language: Judgments of Grammaticalness.
- WALLACE L. CHAFE: Seneca Morphology IV: Expanded Bases.
- MARVIN K. MAYERS: The Linguistic Unity of Pocomam-Pocomchi.
- SYLVIA M. BROADBENT and CATHERINE A. CALLAGHAN: Comparative Miwok: A Preliminary Survey.
- DORIS BARTHOLOMEW: Some Revisions of Proto-Otomi Consonants.
- JACOB A. LOEWEN: Spanish Loanwords in Waunana.

Volume 27, No. 1, January 1961

- A. L. KROEBER: Semantic Contribution of Lexicostatistics.
- ROBERT E. LONGACRE: Swadesh's Macro-Mixtecan Hypothesis.
- DOUGLAS TAYLOR: Some Particular Problems in the Application of the 100-Item Lexicostatistic Test List.
- WALLACE L. CHAFE: Seneca Morphology V: Expanded Aspect Suffixes: Attributive Suffixes.
- WILLIAM SHIPLEY: Maidu and Nisenan: A Binary Survey.

The Japanese Journal of Ethnology (Minzokugaku-Kenkyu)²²

Volume 24, No. 3, 1960

- SHIGERU IJIMA: The Thakali—A Central Himalayan Tribe (Torbo Ethnography No. 1). (English summary).
- RYŪZŌ TAKAYAMA: Economy of the Agropastoral Tibetans in the Torbo Region, N. W. Nepal (Torbo Ethnography No. 2). (English summary).
- KAZUO NUNOMURA: Dual Organization and Marriage between Different Generations in Ancient Japan. (English summary).

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- M. G. LEWIN: Sowjetische ethnographische Forschungen in Nordibirien und im Fernen Osten.
- A. P. OKLADNIKOW: Die alten Kulturen der russische Seeprovinz im Lichte neuer Forschungen.
- MINANO HAYASHI: On the Horse of the Pre-Ch'in Period in China (2).

NORITADA KUBO: On the Koshin Belief—In Reply to the Criticism of Japanese Folklorists.

MASARU MITARAI: La Pensée Cosmographique et Géographique dans la Chine Ancienne.

TAKESHI SHIBATA: Dialect Geography of the Word 'Kataguruma' in the Itoigawa-Oomi District.

SHINJIRO NAGAOKA: History of Kongsî in West Borneo.

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Volume 1, No. 1, 1960

C. K. MEEK: The Niger and the Classics: the history of a name.

JAMES WALTON: Patterned walling in African folk building.

G. S. P. FREEMAN-GRENVILLE: East African coin finds and their historical significance.

J. VANSINA: Recording the oral history of the Bakuba: I. Methods.

C. R. BOXER: S. R. Welch and his history of the Portuguese in Africa, 1495–1806.

N. R. BENNETT: Christian and Negro Slavery in eighteenth-century North Africa.

MARGARET PRIESTLEY and IVOR WILKS: The Ashanti Kings in the eighteenth century: a revised chronology.

J. D. HARGREAVES: Towards a history of the partition of Africa.

C. NEWBURY: The formation of the Government General of French West Africa.

PHILIP D. CURTIN: The archives of tropical Africa: a reconnaissance.

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Volume 73, No. 287, January–March 1960

WAYLAND D. HAND: American Folklore After Seventy Years: Survey and Prospect.

WILLIAM I. SCHREIBER: Amish Wedding Days.

WILLIAM M. JONES: Eudora Welty's Use of Myth in "Death of a Traveling Salesman."

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FRANCIS L. K. HSU: A Neglected Aspect of Witchcraft Studies.

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LEONARD W. MOSS and STEPHEN CAPPANARI: Folklore and Medicine in an Italian Village.

MERLE L. SIMMONS: Pre-Conquest Narrative Songs in Spanish America.

ROBERT STEVENSON: Early Peruvian Folk Music.

MORRIS E. OPLER: Myth and Practice in Jicarilla Apache Eschatology.

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J. L. FISCHER and MARK J. SWARTZ: Socio-Psychological Aspects of Some Trukese and Ponapean Love Songs.

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OTTO BLEHR: On the Communication of Folk Belief in a Situation of Disintegration.

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EDGAR A. LIST: Holda and the Venusberg. GERTRUDE P. KURATH: Calling the Rain Gods.

MARGARET SCHEVILL LINK: From the Desk of Washington Matthews.

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ROGER DUFF: Pacific Adzes and Migrations.

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D. YEN: The Sweet Potato in the Pacific: the propagation of the plant in relation to its distribution.

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MARY DOUGLAS: Blood-debts and Clientship among the Lele.

EMRYS PETERS: The Proliferation of Segments in the Lineage of the Bedouin in Cyrenaica.

J. S. BOSTON: Some Northern Ibo Masquerades.

AUDREY J. BUTT: The Birth of a Religion.

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H. A. POWELL: Competitive Leadership in Trobriand Political Organization.

EDWARD P. DOZIER: The Pueblos of the South-Western United States.

D. K. SEN: Blood Groups and Haemoglobin Variants in some Upper Castes of Bengal.

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AUDREY RICHARDS: Social Mechanisms for the Transfer of Political Rights in Some African Tribes. (Presidential Address.)

DOROTHY EMMET: How far can Structural Studies take account of Individuals?

ROBIN HORTON: A definition of Religion, and its Uses.

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E. E. EVANS-PRITCHARD: Zande Cannibalism.

J. GERAINT JENKINS: Field-Work and Documentation in Folk-Life Studies.

THURSTON SHAW: Early Smoking Pipes: In Africa, Europe, and America.

RAYMOND J. CABANNES: The Distribution of Abnormal Haemoglobins in Algeria, the Hoggar, and High Volta: Anthropological Incidences.

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H. T. NORRIS: The Early Islamic Settlement in Gibraltar.

J. R. HARDING: Conus Shell Disc Ornaments (*Vibangua*) in Africa.

H. CORY: Sumbwa Birth Figurines.

PAULA BROWN: Chimbu Death Payments.

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E. SUNDERLAND: The Anthropometry of the People of Ammanford, South Wales.

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CHARLES A. FERGUSON and MUNIER CHOWDHURY: The phonemes of Bengali

WILLIAM COWAN: Arabic evidence for Proto-Semitic */awa/ and */ô/.

ROBERT E. LONGACRE: String constituent analysis.

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HENRY M. HOENIGSWALD: Phonetic similarity in internal reconstruction.

GUSTAV MUST: A Gaulish incantation in Marcellus of Bordeaux.

ERNST PULGRAM: New evidence on Indo-European names.

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R. B. LEES: A multiply ambiguous adjectival construction in English.

ANDRÉ MALÉCOT: Vowel nasality as a distinctive feature in American English.

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VAKOV MALKIEL: Paradigmatic resistance to sound change.

WARREN COWGILL: Greek *ou* and Armenian *oc*.

BROTHER DAVID H. KELLY, F.S.C.: Internal reconstruction of juncture for Proto-Greek.

RICHARD P. SEYMOUR: Reflexes of French *mécanique* in German dialects.

ROBERT P. STOCKWELL: The place of intonation in a generative grammar of English.

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KEITH PERCIVAL: A problem in competing phonemic solutions.

PETER LADEFOGED: The value of phonetic statements.

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Volume 60, Articles 27–50, February 1960

RAYMOND FIRTH: Tikopia Woodworking Ornament.

ELLIOTT P. SKINNER: The Mossi "Pog-sioute."

Volume 60, Articles 94–117, May 1960

DOUGLAS FRASER: African Masks from an Unrecorded Style Province.

HENRY ROSENFELD: On Determinants of the Status of Arab Village Women.

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C. P. MOUNTFORD: Phallic Objects of the Australian Aborigines.

MAURICE FREEDMAN: A Note on Social Organization in a Rural Area of Greater Djakarta.

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G. DE G. SIEVEKING: Morhana Pahar: or the Mystery of A. C. Carlyle.

- E. E. EVANS-PRITCHARD: The Ethnic Origins of Zande Office-Holders.
C. M. N. WHITE: Land Tenure and Village Structure among the Luvale.

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- BERNARD FAGG and WILLIAM FAGG: The Ritual Stools of Ancient Ife.
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H. GRAHAM CARR: Some Dental Characteristics of the Middle Minoans.

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- M. R. A. CHANCE: Kohler's Chimpanzees—How Did They Perform?
J. L. JENKINS: The *Morienhur*: A Mongolian Fiddle.
J. R. HARDING: On Some Crucibles and Associated Finds from the Coast of Tanganyika.

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- I. MORGAN WATKIN: A Viking Settlement in Little England beyond Wales: ABO Blood Group Evidence.
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- THURSTAN SHAW: Excavations at Igbo-Ukwu, Eastern Nigeria: An Interim Report.
S. KOOLJMAN: A Papuan Lunar 'Calendar': The Reckoning of Moons and Seasons by the Marind-Anim of Netherlands New Guinea.

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- DONALD F. THOMSON: A Bark Sandal from the Desert of Central Western Australia.

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- VINIG L. GROTTFANELLE: Pre-Existence and Survival in Nzema Beliefs.
DONALD F. THOMSON: A Narcotic from 'Nicotiana ingulba.' Used by the Desert Bindibu.
W. R. G. HORTON: Social Science, Logical or Psychological Impossibility?

Volume 61, Articles 26-47, February 1961

- KENELM BURRIDGE: "Kuda Kepang" in Batu Pahat, Johore.
VERNON R. DORJAHN: The Initiation of Temne 'Poro' Officials.

Man in India²⁷

Volume 40, No. 1, January-March 1960

- NITYANANDA PATNAIK: Outcasting among Oḥmer for Drinking Wine.
SAJANI KANTA DAS: Regulations for Kshatriyas from Hindustan Residing in Bengal.
D. P. SINHA: Caste Dynamics: A Case from Uttar Pradesh.
MANMATHA NATH DAS: Female Infanticide among the Khonds of Orissa.
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- PRABODHI KUMAR BHOWMICK: Four Temples in Midnapur, West Bengal.
NITYANANDA PATNAIK: Barbers and their Clients in an Orissan Village.
BRIJ RAJ CHAUHAN: An Indian Village: Some Questions.
BHUBAN M. DAS: Somatic Variations among the Hill and Plains Gavo of Assam.
JATINDRA MOHAN DATTA: Proportion of Uttara-Rathi Kayasthas among Kayasthas of Bengal.

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- GAUTAMSANKAR RAY: Prehistory of Orissa.
DHARANI SEN and ASOK KUMAR GHOSH: On the Occurrence of Palaeoliths in Singhbhum.
SCARLETT EPSTEIN: Economic Development and Peasant Marriage in South India.
S. N. SANYAL: The Approach to the Problem of Population and to its Solution in India.

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- J. A. BARNES: Anthropology in Britain Before and After Darwin.
F. D. MCCARTHY and F. HANSEN: Rock Engravings, Sydney District: Nos. 72-102.
C. P. MOUNTFORD: Unusual Rock Markings in Australia.

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Volume 31, No. 1, September 1960

- RALPH BULMER: Political Aspects of the Moka Ceremonial Exchange System among the Kyaka People of the Western Highlands of New Guinea.
E. A. HAMMOND: Some Models for the Analysis of Marriage-Section Systems.
A. CAPILL: Myths and Tales of the Nungubuyu, S.E. Arnhem Land.
W. E. HARNEY: Ritual and Behaviour at Ayers Rock.
R. J. WALSH, J. L. JAMESON, and OLGA KOOPITZOFF: Blood Groups and Haemoglobin Values of Natives from Minj, New Guinea.

Paideuma³⁰

Volume 7, No. 3, May 1960

- RÜDIGER SCHOTT: Religiöse und soziale Bindungen des Eigentums bei Naturvölkern.
MEINHARD SCHUSTER: Zur Diskussion des Megalithproblems.
EIKE HABERLAND: Besessenheitskulte in Süd-Äthiopien.
HORST NACHTIGALL: Zur Entstehung der amerikanischen Hochkulturen.
MIRKO KUS-NIKOLAJEV: Zur Symbolik des altkroatischen Bendgelflechts.

Volume 7, Nos. 4-6, July 1960 (Festgabe für Herman Lommel)

- HEINZ BECHERT: Mutterrecht und Thronfolge in Malabar und Ceylon.
EMILE BENVENISTE: Les noms de l' "oiseau" en Iranien.
GEORG BUDDRUS: Zur Mythologie der Prasun-Kafiren.
J. DUCHESNE-GUILLEMIN: De la dicéphalie dans l'iconographie mazdéenne.
GEORGE DUMÉZIL: A propos de quelques représentations folkloriques des Ossètes.
MIRCEA ELIADE: Spiritual Thread, Sūtrātman, Catena Aurea.
HELMUTH VON GLASENAPP: Hat Buddha ein metaphysisches System gelehrt?
CLAUS HÄBLER: Ausschnidekunst im alten Indien.
HELMUT HUMBACH: Der iranische Mithra als Daiva.
AD E. JENSEN: Feld-Tettassen und Megalithen.
MANFRED MAYRHOFER: Mongolische Pferdewörter bei einem Sanskrit-Lexikographen?
WILFRIED NÖLLE: Awestisch "Chshathra varya."
HELMUT RAHN: Die Frömmigkeit der Kyniker.
WILHELM RAU: Bemerkungen zu Śaṅkaras Brhadāraṇyāṅkopaṇiṣadbhāṣya.

- LOUIS RENO: Varuna dans l'Atharvaveda.
BERNFRIED SCHLERATH: Opfergaben.
PAUL THIEME: Besetzung in Sprache, Dichtung und Religion.

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- MAKOTO SUZUKI, and TOKURO SAKAI: Mesiolingual Randtubercle of Permanent Maxillary Molars in Japanese.
KOZI TUBAKI, and KATUO SINDO: On the Morphology of the Eyelid in Japanese People. Age-change of the Forms of Eyelid.
TAMOTSU OGATA: Einige Fragen über die Kranio-oder die Osteometrie des Menschen, besonders über den Collo-Diaphysenwinkel des Femur.
MOTOSUGU KOHAMA: An Anthropometrical Study of the Japanese. On the Local Differences and the Origin of the Japanese.
HISAKATSU HIDIKATA: Ceramics with Decoration found in the Island Palao, Micronesia.
YUTAKA IMAMURA: Physical Sex-difference and Growth of Boys and Girls.
GORO SHIMA: On the Peculiarity of Toe-prints and the Classification of Toe-pattern Types.
MASACHIKU KUTSUNA: Anthropological Studies on the Inhabitants of Shikoku District, Japan.
GISUKE KIDO: Anthropological Studies on the Inhabitants of Kuroki-cho, Yame-gun, Fukuoka, Prefecture, Japan.
TEH-MAW YUAN and CHIAN-SHIN TSUNG: Kranimetrie der Battak und andere Eingeborenen Sumatras.
MIKUNI TERASHI and ITOH MISHIMA: On the Sub-burial Toweizers and Cowrie Shells in Cinerary Urn, at Isa-gun, Okuchi, Satsuma.
TERUHIKO SEMBA: The Studies on the Bones of Animals from the Sites of Middle and Later Yayoi Period in the Iki Island, Nagasaki Prefecture, Japan.

Revista de Antropologia³²

Volume 8, No. 1, June 1960

- P. H. SALDANHA, O. FROTA-PESSOA, PHYLLIS EVELETH, F. OTTENSOPFER, ALDA B. CUNHA e MARINA A. A. CAVALCANTI: Estudo genético e antropológico de uma colônia de holandeses do Brasil.
GERMÁN FERNÁNDEZ GUZZETTI: Proyecciones filosóficas de algunas teorías etnolingüísticas contemporáneas.
MARIA ISAUARA PEREIRA DE QUEIROZ: Aspectos gerais do messianismo.

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Volume 16, 1960

- RITA LÓPEZ DE LLERGO: Principales rasgos fisiográficos de la región comprendida entre el paralelo 19° y el Istmo de Tehuantepec.
HELIA BRAVO H.: Algunos datos acerca de la vegetación del Estado de Oaxaca.
JOSÉ L. LORENZO: Aspectos físicos del Valle de Oaxaca.
ROMÁN PIÑA CHAN: Algunos sitios arqueológicos de Oaxaca y Guerrero.
LAURETTE SÉJOURNÉ: El simbolismo de los rituales funerarios en Monte Albán.
JOHN PADDOCK: Exploración en Yagul, Oaxaca.

EDUARDO PAREYÓN MORENO: Exploraciones arqueológicas en Ciudad Vieja de Quíotepec, Oaxaca.

AGUSTÍN DELGADO: Exploraciones en la Chinantla.

JOSÉ GARCÍA PAYÓN: Correlaciones arqueológicas del Centro de Veracruz y Oaxaca.

EDUARDO NOGUERA: Relaciones de Oaxaca con Puebla y Tlaxcala: Culturas Cholulteca, Mixteca y Zapoteca.

MA. TERESA FERNÁNDEZ DE MIRANDA, MAURICIO SWADESH y ROBERTO J. WEITLANER: El panorama Etno-Lingüístico de Oaxaca y el Istmo.

WOODROW BORAH: Sources and possibilities for the reconstruction of the demographic process of the Mixteca Alta, 1519-1895.

SHERBURNE F. COOK: Reconstruction of extinct population.

R. J. WEITLANER y S. HOOGSHAGEN: Grados de edad en Oaxaca.

RICARDO POZAS: Etnografía de los Mazatecos.

RALPH L. BEALS: Algunos aspectos de la Aculturación Mixe.

RODOLFO STAVENHAGEN: Descendencia y nombres entre los Mazatecos.

JULIO DE LA FUENTE: La cultura Mazateca. SEARLE HOOGSHAGEN: Elección, instalación y aseguramiento de los funcionarios en Coatlán.

JUAN A. HASLER: El mundo físico-espiritual de los Mazatecos de Ichatlán.

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Number 27, June 1960

DAVID G. BETTISON: The Poverty Datum Line in Central Africa.

THAYER SCUDDER: Fishermen of the Zambezi.

C. FRANTZ: Changes and Continuities in Africa: a review article.

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Volume 10, No. 1, 1960

RICHARD THURNWALD: Die Probleme einer empirischen Soziologie.

WILLIAM C. LEHMANN: Herder's Contribution toward an Empirical Sociology and Cultural Anthropology.

RUDOLF BERGIUS: Sozialpsychologische Probleme der Kindes- und Jugendentwicklung.

CATHERINE H. BERNDT: The Concept of Primitive.

OSWIN KÖHLER: Sprachkritische Aspekte zur Hamitentheorie über die Herkunft der Hottentotten.

Volume 10, No. 2, 1960

RODNEY NEEDHAM: Alliance and Classification among the Lamet.

KARL JETTMAR: Soziale und wirtschaftliche Dynamik bei asiatischen Gebirgsbauern.

WOLFRAM EBERHARD: Modern Tendencies in Islam in Pakistan.

SIGRID WESTPHAL-HELLBUSCH: Umwelt und historischer Zufall als Faktoren des Kulturwandels im Südiraq.

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Volume 16, No. 1, Spring 1960

A. CAPELL: Language and World View in Northern Kimberley, Western Australia.

JAMES T. DAVIS: An Appraisal of Certain Speculations on Prehistoric Puebloan Subsistence.

PAULA BROWN: Chimbu Tribes: Political Organization in the Eastern Highlands of New Guinea.

PHILIP DRUCKER and ROBERT F. HEIZER: A Study of the Milpa System of La Venta Island and its Archaeological Implications.

ETHEL M. ALBERT: Socio-political Organization and Receptivity to Change: Some Differences Between Ruanda and Urundi.

WILLIAM S. LAUGHLIN: Aspects of Current Physical Anthropology: Method and Theory.

ROBERT T. ANDERSON and GALLATIN ANDERSON: Sexual Behavior and Urbanization in a Danish Village.

ALBERT HEINRICH: Structural Features of Northwestern Alaskan Eskimo Kinship.

Volume 16, No. 2, Summer 1960

RICHARD W. LIEBAN: Sorcery, Illness, and Social Control in a Philippine Municipality.

NANCIE L. SOLIEN: Changes in Black Carib Kinship Terminology.

E. H. SELLARDS: Some Early Stone Artifact Developments in North America.

WILLIAM HENRY SCOTT: Social and Religious Culture of the Kalingas of Madukayan.

DAVID MAYBURY-LEWIS: Parallel Descent and the Apinayé Anomaly.

JAMES H. HOWARD: When They Worship the Underwater Panther: A Prairie Potawatomi Bundle Ceremony.

FLOYD THURBER and VALERIE THURBER: A Maya Hieroglyph Possibly Symbolizing Thunder.

HENRY F. DOBYNS, PAUL H. EZELL, ALDEN W. JONES, and GRETA S. EZELL: What Were Nixoras?

Volume 16, No. 3, Autumn 1960

T. D. STEWART: A Physical Anthropologist's View of the Peopling of the New World.

RODNEY NEEDHAM: Patrilineal Prescriptive Alliance and the Ungarinyin.

MISCHA TITIEV: A Fresh Approach to the Problem of Magic and Religion.

WILLIAM L. ROWE: The Marriage Network and Structural Change in a North Indian Community.

HUGH CUTLER and GEORGE A. AGOGINO: Analysis of Maize from the Four Bear Site and Two Other Arikara Locations in South Dakota.

DAVID H. KELLEY: Calendar Animals and Deities.

BRUNO NETTL: Musical Cartography and the Distribution of Music.

R. S. KHARE: The Kānya-Kubja Brahmins and their Caste Organization.

CLINTON R. EDWARDS: Sailing Rafts of Sechura: History and Problems of Origin.

Volume 16, No. 4, Winter 1960

MARC J. SWARTZ: Situational Determinants of Kinship Terminology.

THOMAS O. BALLINGER and PURNA HARSHA BAJRACHARYA: Nepalese Musical Institutions.

MARIA ROSTWOROWSKI DE DIEZ CANSECO: Succession, Cooption to Kingship, and Royal Incest among the Inca.

HARRIETTE H. THOMSEN: Occurrence of Fired Bricks in Pre-Conquest Mexico.

LORENZ G. LOFFLER: The Development of the Ambrym and Pentecost Kinship Systems.

SCHUYLER CAMMANN: Toggles and Toggle-wearing.

HAROLD A. GOULD: The Micro-demography of Marriages in a North Indian Area.

RICHARD G. SNYDER: Hyperodontia in Prehistoric Southwest Indians.

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No. 1, 1960

B. KH. KARMYCHEVA (Moscou). Un groupe ethnographique "turc" parmi les Ouzbeks.

TCH. M. TAKSAMI (Leningrad). Les villages et demeures des Nivkhs contemporains.

B. L. RIFTINE (Moscou). Le folklore contemporain chinois.

S. A. AROUTOUNOV (Moscou). De l'appréciation du rôle des migrations dans l'histoire ancienne du Japon.

N. A. BOUTINOV (Leningrad). "Les oreilles longues" et "les oreilles courtes" à l'île de Pâques.

M. V. TCHOURAKOV (Leningrad). "Nouvelle Histoire du Nedjed" d'Amin ar Reichani comme source ethnographique sur l'Arabie Centrale.

M. G. JOURAVLEVA (Moscou). Les Chinois de la Fédération Malaise.

No. 2, 1960 (Pour le 90^{ml} anniversaire de W. I. Lénine)

T. A. JDANKO (Moscou). La politique nationale de Lénine à une nouvelle étape historique. (Sur el problème de l'évolution des nations socialistes de l'Asie Centrale vers le communisme).

L. P. POTAPOV (Leningrad). Les problèmes des études ethnographiques sur les peuples de Sibérie à la lumière de la théorie de Lénine sur la question nationale.

V. BÉLITZER et T. SMIRNOVA (Moscou). Pour le 30^{ml} anniversaire de la R.A.S.S. Mordouane.

V. A. ALEXANDROV (Moscou). Les demeures russes en Sibérie Orientale au XVII^{ml}—commencement du XVIII^{ml} siècle.

G. V. CHIPOUNOVA (Leningrad). L'art populaire des Komis.

L. A. FADÉEV (Moscou). Le problème de l'origine de la culture Zimbabwé.

V. A. LIVCHITZ (Leningrad). L'ambassadeur sogdien à Tchatcha (Document A-14 du mont Moug).

T. A. TROFIMOVA (Moscou). Les principaux résultats et les problèmes des études paléanthropologiques de l'Asie Centrale.

A. D. DRIDZO (Leningrad). La population de Cuba.

No. 3, 1960 (Vingt ans des Républiques Socialistes Soviétiques Baltes)

H. A. et A. H. MOORA (Tallin). Sur la question des régions historico-culturelles de la Baltique.

A. K. KRASTYNIA (Riga). De la construction des habitations des kolkhosiens en R.S.S. de Lettonie.

L. EFRÉMOVA (Riga). Quelques résultats des études ethnographiques des kolkhosienspêcheurs de Vidzémé.

Y. V. MARÉTINE (Leningrad). Disparition des survivances et vestiges du matriarcat dans les relations conjugales chez les Minangkabaou.

N. N. TCHEBOKSAROV (Moscou). Des liens économiques et culturels des peuples Baltes.

M. G. LÉVINE (Moscou) et D. A. SERGUÉIEV (Leningrad). Du temps de la pénétration du fer dans l'Arctique. (La première trouvaille d'un instrument en fer dans un vestige de la culture arctique des bords de la Mer de Behring.)

A. B. DAVIDSON (Moscou). Discrimination des Africains à l'Union Sud-Africaine.

No. 4, 1960 (La fête des peuples de la région du Volga)

V. K. SOKOLOVA (Moscou). Le folklore comme source historico-ethnographique.

- B. N. POUTILOV (Leningrad). Problèmes courants de l'histoire du folklore russe.
 R. LIPETZ et M. RABINOVITCH (Moscou). Sur la question du temps de composition des "bylines." (Les armes des "bogatyrs")
 V. P. ANIKINE (Moscou). Sur la recherche de l'époque de création des proverbes, des dictons et des énigmes.
 N. POLICHTCHOUK (Pétrozavodsk). Les chansons des mineurs comme matériaux pour l'étude de la situation et du genre de vie des ouvriers.
 E. M. MELÉTSKY (Moscou). Sur la question de la genèse de l'épopée carelo-finnoise. (Le problème de Viainiainen).
 S. N. PLOUJNIKOVA (Moscou). Les images d' "ossilkis" dans les mythes et contes bielorusses.
 E. A. ALEXÉENKO (Leningrad). Le culte de l'ours chez les Kètes.
 B. RIFTINE (Moscou). Les proverbes chinois comme matériaux pour l'étude de la philosophie, des croyances et de la morale sociale du peuple.
 Z. I. VLASSOVA (Leningrad). Les intérêts ethnographiques de V. G. Korelenko.

Voprosy Yazykoznamiya²⁷

No. 4, 1960

- V. A. AVORIN (Leningrad): The Lenin national politics and the development of literary languages of the peoples of the U.S.S.R.
 T. LEHR-SPLAWIŃSKI (Krakow): The problem of the origins of the Slavs and its modern development.
 B. V. GORNUNG (Moscow): On the place of linguistics in the system of sciences and the application in linguistics of methods used in other sciences.
 E. B. AGAYAN (Yerevan): On the genesis of Armenian consonantism.

Notes

- ¹ Postafiók 440, Budapest 62, Hungary.
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- M. A. ČERKASSKIJ (Alma-Ata): On the genesis of synharmonic variants and parallelisms in the Turk languages.
 B. A. SEREBREUNIKOV (Moscow): On some moot problems of comparative and historical phonetics of the Turk language.

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- V. V. VINOGRADOV (Moscow): On homonymy and related problems.
 S. K. ŠAUMJAN (Moscow): The two-stage theory of the phoneme and of the differential elements.
 V. I. GEORGIEV (Sofia): The mutation of occlusive consonants in Armenian and problems of the ethnogenesis of the Armenians; On the formation of East Slavonic national literary languages.

No. 6, 1960

- A. DOSTAL (Prague): On the study of word-stock in Old Church Slavonic.
 N. Y. ŠVEDOVA (Moscow): The importance of lexical limitations for the history of Russian syntax in the XVIII-XIX centuries.
 R. G. PIOTROVSKI (Leningrad): Once more on distinctive features of the phoneme.
 G. D. DJAUKJAN (Yerevan): On the origin of consonantism in Armenian dialects.
 V. Z. PANFILOV (Leningrad): The problem of the word and the "incorporation" in the Nivkh language; On the formation of East Slavonic national literary languages.

Word

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- HENRI FRET: Tranches homophones.
 WALTER ARNDT: "Modal Particles" in Russian and German.

- JOHN W. MARTIN: Remarks on the Origin of the Portuguese Inflected Infinitive.
 MURRAY FOWLER: Stress-Determined Allophones in English.
 JOHN T. BENDOR-SAMUEL: Some Problems of Segmentation in the Phonological Analysis of Tereno.
 ANNA GRANVILLE HATCHER: An Introduction to the Analysis of English Noun Compounds.

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- CARL A. SCHMITZ: Verwandtschaftsnamen und Kulturschichten im Nordosten von Neuguinea.
 HERMA PLAZIKOWSKY-BRAUNER: Über die Wallamö.
 WERNER LANG: Makondemasken in der völkerkundlichen Sammlung der Universität Göttingen.
 MARIO YPIRANGA MONTEIRO: Cariamã, Pubertätsritus der Tucano-Indianer.
 MIRKO KUS-NIKOLAJEV: Über die soziologischen Grundlagen der montenegrischen humanitas heroica.
 SIEGBERT HUMMEL: Die Frauenreiche in Tibet. (Eine Randbemerkung zu M. Hermanns: Die Familie der A Mdo-Tibeter).
 MIRKO BARJAKTAROVIĆ: Das leere Grab—ein alter Brauch in Serbien.
 WANDA HANKE: Die Sprache der Kaišana am Lago Mapari.
 IVAR PAULSON: Die Schutzgeister und Gottheiten der Jagdtiere im Glauben der nordasiatischen (sibirischen) Völker.

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³⁵ Verlag Duncker und Humboldt, Geraniensstrasse 2, Berlin-Lichterfelde, Germany.

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Publications Received

The Editor wishes to acknowledge with thanks the receipt of the publications listed below. Hereafter, *Publications Received* will include only major publications by Associates, including books, monographs, and articles that bear a date 1959 or later and that are not included in the journal listings that will be printed systematically beginning with this issue of CA.

To make our listings more accurate and serviceable, each publication sent to the Editor should include on the cover or title page complete information (with date, city, etc.), translated into English, and, where necessary, transliterated into roman script.

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THE INTERNATIONAL DIRECTORY OF ANTHROPOLOGICAL INSTITUTIONS

Steps Toward a New Edition

The *International Directory of Anthropological Institutions*, edited by William L. Thomas, Jr. and Anna M. Pikelis, was published in 1953 by the Wenner-Gren Foundation for Anthropological Research, Inc., and was distributed by the American Anthropological Association. Issued as a handbook of world resources for research and education in Anthropology, the *Directory* consisted "primarily of succinct descriptions of the organization and operation of all institutions with interests in anthropology, comprised of Educational Institutions (colleges, universities, academies, schools, institutes); Museums and Research Institutions; Professional Associations (local, national, and international); and Agencies (private and government) subsidizing anthropological research." The material was organized in 27 sections, corresponding to countries or geographical areas, each of which was the responsibility of a contributing editor, who gathered and arranged the data and provided an introduction reviewing his or her area.

In the nine years that have passed since the material for the *Directory* was gathered, the expansion of the anthropological sciences has combined with sweeping political changes to render sections of the work obsolete. Clearly, a new edition is needed. While the plans for issuing a revision of the directory are still being discussed, two initial and indispensable steps can be taken now: (1) obtaining a complete and up-to-date list of institutions to be included in the new edition; and (2) preparing questionnaires to be sent to these.

Consequently, we are reproducing below a list of the institutions and organizations included in the 1953 *Directory*. The original organization has been retained; and except for a few areas where changes have been so extensive that it seems best to omit all of the previous listings, they are listed without change. Suggestions for additions, changes, or deletions that should be made may be sent to the Editor of CA.

On the opposite page are reproduced, for reference and discussion, the questionnaires used in gathering the information for the *Directory*.

1953 Directory Listings

International

THE ARCTIC INSTITUTE OF NORTH AMERICA, Montreal, Canada; Washington and New York, U.S.A.

ASSOCIATION OF CARIBBEAN ARCHAEOLOGISTS, Havana, Cuba.

COMMISSION OF HISTORY OF THE PAN AMERICAN INSTITUTE OF GEOGRAPHY AND HISTORY, Mexico D.F., Mexico.

INTER-AMERICAN INDIAN INSTITUTE, Mexico, D.F., Mexico.

INTERNATIONAL AFRICAN INSTITUTE, London, England.

INTERNATIONAL ASSOCIATION ON QUATERNARY RESEARCH (INQUA), Pisa, Italy.

INTERNATIONAL CONFERENCE OF WEST AFRICANISTS, Dakar, Senegal.

INTERNATIONAL CONGRESS OF AMERICANISTS.

INTERNATIONAL CONGRESS OF ANTHROPOLOGICAL AND ETHNOLOGICAL SCIENCES.

INTERNATIONAL CONGRESS OF PREHISTORIC AND PROHISTORIC SCIENCES, Ghent, Belgium.

INTERNATIONAL COUNCIL FOR PHILOSOPHY AND HUMANISTIC STUDIES (CIPSH), Brussels, Belgium.

INTERNATIONAL INSTITUTE OF DIFFERING CIVILISATIONS (INCIDI), Brussels, Belgium.

INTERNATIONAL UNION OF ANTHROPOLOGICAL AND ETHNOGRAPHICAL SCIENCES (UISAE), Paris, France.

PACIFIC SCIENCE ASSOCIATION, Honolulu, Hawaii, U.S.A.

PAN-AFRICAN CONGRESS OF PREHISTORY, Algiers, Algeria.

PAN AMERICAN INSTITUTE OF GEOGRAPHY AND HISTORY, Mexico, D.F., Mexico.

SCIENTIFIC COUNCIL FOR AFRICA SOUTH OF THE SAHARA (CSA), Kikuyu, Kenya.

SOUTH PACIFIC COMMISSION, Anse Vatu, New Caledonia.

TRUST TERRITORY OF THE PACIFIC ISLANDS, Honolulu, Hawaii, U.S.A.

UNITED NATIONS EDUCATIONAL, SCIENTIFIC, AND CULTURAL ORGANIZATION, Paris, France.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH INCORPORATED, New York, U.S.A.

Fiji

FIJI MUSEUM, Suva.

THE FIJI SOCIETY, Suva.

New Caledonia

FRENCH OCEANIA INSTITUTE, Noumea.

NEW CALEDONIAN MUSEUM, Noumea.

SOCIETY OF MELANESIAN STUDIES, Noumea.

Papua and New Guinea

THE PAPUA AND NEW GUINEA SCIENTIFIC SOCIETY, Port Moresby.

Tahiti

PAPEETE MUSEUM, Papeete.

SOCIETY OF OCEANIC STUDIES, Papeete.

Australia

ANTHROPOLOGICAL SOCIETY OF NEW SOUTH WALES, Sydney, N.S.W.

ANTHROPOLOGICAL SOCIETY OF QUEENSLAND, Brisbane, Queensland.

ANTHROPOLOGICAL SOCIETY OF SOUTH AUSTRALIA, Adelaide, South Australia.

(Continued on page 288)

PUBLICATIONS RECEIVED

(Continued from page 285)

WOLAŃSKI, NAPOLEON, Sferodorsimetr—własnego pomysłu przyrząd do dokonywania przestrzennych pomiarów kęśłupa ("The Spherodorsimeter—an instrument invented by the author for spatial measurements of the spinal column"). *Zeszyty Naukowe Uniwersytetu Jagiellońskiego, Zoologia* (1957) NR 10: 242-57.

—, Jeszcze w sprawie skokowości w ewolucji form ludzkich oraz nierównomierności tempa rozwoju ("Further observations on sudden changes in the

evolution of human forms and unevenness in the rate of development"). *Przegląd Antropologiczny* (1957) 23: 441-45.

—, Zagadnienie etapowości w ewolucji form ludzkich ("The problem of the stages in the evolution of human forms"). *Przegląd Antropologiczny* (1958) 24: 308-33.

—, Výška těla polských hochů v posledním půstoletí ("Growth in height of Polish boys in the last fifty years"). *Zpráva o činnosti Anthropologické společnosti* (1958) 11:1-2.

—, Nowe punkty antropologiczne na człowieku żywym ("New anthropological points on the living"). *Przegląd Antropologiczny* (1958) 24:537-39.

—, Asymetria ciała człowieka i jej ("Asymmetry in the human body and its variability in the light of limb function"). *Przegląd Antropologiczny* (1957) 23: 461-64.

ZIMMERMANN, GÜNTHER, *Das Geschichtswerk des Domingo de Muñon Chimalpahin Quauhtlehuanitzin*. 77 pp. Hamburg: Museums für Völkerkunde und Vorgeschichte, 1960.

VAN ZINDEREN BAKKER, E. M. (Ed.), *Paleontology in Africa, Sixth Report* (covering the years 1948 and 1959). 40 pp. Bloemfontein: Andrew & Co., 1960.

1953 Questionnaires

Questionnaires to be used in the compilation of data, and a cover letter explaining the scope and aims of the project, were prepared in quantity by the Wenner-Gren Foundation and distributed to compiling editors in numbers requested by them. These are reproduced on the following pages, and are accompanied by an explanation of principles and method applied by the editors to the condensation and standardization of data received.

General

All data, except those items on chief officer(s) of institution, nature and date of founding, and governing body, pertain solely to implementation of *anthropological* teaching and research.

The English language has been employed throughout, other languages being retained only in the case of publications titles or of technical terms; where meaning is obscured by the practice of using English alone, the original language follows in parentheses. American spellings have been consistently employed except in the case of the hobgoblin "ae" versus "e," e.g., "arch-(a)ecology."

Monetary items are given in the currency of the country in which an institution is located; equivalents in terms of the U.S.A. dollar are provided in most instances, according to foreign exchange quotations of the Manufacturers Trust Company in New York City (1 May 1952).

Where possible, academic degrees and titles have been equated with those commonly found in institutions in the U.S.A.

"Publications" has been defined as serial titles currently published by the institution in question, although general terms have also been employed to cover considerable publication in other media.

"Affiliations" has been defined as institutional affiliation on an operating level and excludes association by virtue of the personal activities of staff members or of library subscription; in the absence of precise detail this distinction has sometimes proved a very difficult one.

Plans, undocumented as to early concrete implementation, have been omitted from "Other Activities."

All data were collected during 1952 and phrases such as "to date," "present," and the like should be understood in this context. Specific dates, when submitted, are given.

QUESTIONNAIRE 1, for Educational Institutions

NAME:
ADDRESS:
PRINCIPAL ADMINISTRATIVE OFFICER (title and name):
NATURE AND DATE OF FOUNDING OR ESTABLISHMENT:
TYPE OF GOVERNING BODY OR ADMINISTRATION:
DEPARTMENTS, DIVISIONS, FACULTIES, SCHOOLS, INSTITUTES CONCERNED WITH ANTHROPOLOGY (with title and name of official head of each):
FACULTY ORGANIZATION (titles and number of positions, names of present staff, subjects taught by each, principal teaching specialties, principal research interests):
CALENDAR (numbers and dates of terms):
LANGUAGE OF INSTRUCTION:
ADMISSION OR ENTRANCE REQUIREMENTS:
FEES:
DEGREES, DIPLOMAS, CERTIFICATES OFFERED:

ENROLLMENT:
TOTAL IN INSTITUTION:
TOTAL IN ANTHROPOLOGY:
NUMBER OF CANDIDATES FOR DEGREES IN ANTHROPOLOGY:
RESEARCH FACILITIES (availability to students; availability to visiting scholars):
LABORATORY:
LIBRARY:
MUSEUM:
EQUIPMENT:
FINANCIAL RESOURCES FOR RESEARCH SUBSIDIES TO STAFF (to faculty; to advanced students):
PUBLICATIONS (titles, editors, and frequency of journals or monograph series; principal interests):
AFFILIATIONS WITH OTHER INSTITUTIONS OR ASSOCIATIONS:
OTHER ACTIVITIES (with emphasis on recent developments):

QUESTIONNAIRE 3, for Professional Associations

NAME:
ADDRESS OF CENTRAL OFFICE:
PRINCIPAL OFFICERS (titles; term of tenure; whether elective, honorary, or appointive; names of present officers):
TYPE OF GOVERNING BODY OR ADMINISTRATION:
NATURE AND DATE OF FOUNDING:
PURPOSE OF ORGANIZATION:
MEETINGS (places, frequency, dates, general nature):
MEMBERSHIP (qualifications, costs, number of members, fellows, associates, etc.):
PUBLICATIONS (titles of series, editors, and frequency of journals or monographs; principal interests):
AFFILIATIONS WITH OTHER INSTITUTIONS OR ASSOCIATIONS:
OTHER ACTIVITIES (with emphasis upon recent developments):

QUESTIONNAIRE 2, for Museum or Research Institutions

NAME:
ADDRESS:
PRINCIPAL ADMINISTRATIVE OFFICER (title and name):
NATURE AND DATE OF FOUNDING OR ESTABLISHMENT:
TYPE OF GOVERNING BODY OR ADMINISTRATION:
DEPARTMENTS, DIVISIONS, INSTITUTES CONCERNED WITH ANTHROPOLOGY (with title and name of official head of each):
STAFF ORGANIZATION—full and part-time (titles and number of positions, names of present staff; principal duties and research interests of each):
PRINCIPAL ACTIVITIES OF INSTITUTION AND DEPARTMENTS (exhibits and/or research):

NUMBER, FREQUENCY, AND LEVELS OF INTEREST OF ANTHROPOLOGICAL EXHIBITS:
The COLLECTIONS, PERTAINING TO ANTHROPOLOGY, FOR WHICH THE INSTITUTION IS BEST KNOWN (not a total catalogue):
RESEARCH FACILITIES (for staff members; for visiting scholars):
LABORATORY:
EQUIPMENT:
LIBRARY:
FINANCIAL RESOURCES FOR RESEARCH SUBSIDIES TO STAFF:
PUBLICATIONS (titles, editors, and frequency of journals or monograph series; principal interests):
AFFILIATIONS WITH OTHER INSTITUTIONS OR ASSOCIATIONS:
OTHER ACTIVITIES (with emphasis on recent developments):

QUESTIONNAIRE 4, for Agencies Subsidizing Research

NAME:
ADDRESS:
PRINCIPAL EXECUTIVE OFFICER (title and name):
TYPE OF GOVERNING BODY OR ADMINISTRATION:
AWARDS OR SUBSIDIES AVAILABLE (types—grants, fellowships, scholarships, gifts; number of each, amounts, when awarded):
LIMITATIONS (fields or topics of specialization; regions or countries of specialization; availability to nationals or foreign scholars):
METHOD OF APPLICATION OR PRESENTATION OF PETITION:
FINANCIAL RESOURCES (if available):
PUBLICATIONS:
OTHER ACTIVITIES (conferences, lectures, exhibits, awards, etc.):
AFFILIATIONS WITH OTHER INSTITUTIONS:

International Dictionary

(Continued from page 286)

ANTHROPOLOGICAL SOCIETY OF VICTORIA, Melbourne, Victoria.
 AUSTRALIAN INSTITUTE OF ANATOMY, Canberra, A.C.T.
 THE AUSTRALIAN MUSEUM, Sydney, N.S.W.
 AUSTRALIAN NATIONAL UNIVERSITY, Canberra, A.C.T.
 COMMITTEE ON ANTHROPOLOGY, AUSTRALIAN NATIONAL RESEARCH COUNCIL, Sydney, N.S.W.
 NATIONAL MUSEUM OF VICTORIA, Melbourne, Victoria.
 QUEEN VICTORIA MUSEUM, Launceston, Tasmania.
 QUEENSLAND MUSEUM, Brisbane, Queensland.
 SCHOOL OF PACIFIC ADMINISTRATION, Mosman, N.S.W.
 SOUTH AUSTRALIAN MUSEUM, Adelaide, South Australia.
 TASMANIAN MUSEUM AND ART GALLERY, Hobart, Tasmania.
 UNIVERSITY OF ADELAIDE BOARD FOR ANTHROPOLOGICAL RESEARCH, Adelaide, South Australia.
 UNIVERSITY OF MELBOURNE, Carlton, Victoria.
 UNIVERSITY OF QUEENSLAND, Brisbane, Queensland.
 UNIVERSITY OF SYDNEY, Sydney, N.S.W.
 WESTERN AUSTRALIAN MUSEUM, Perth, Western Australia.

New Zealand

AUCKLAND INSTITUTE AND MUSEUM, Auckland.
 AUCKLAND UNIVERSITY COLLEGE, Auckland.
 CANTERBURY MUSEUM, Christchurch.
 DOMINION MUSEUM, Wellington.
 OTAGO MUSEUM, Dunedin.
 THE POLYNESIAN SOCIETY, INC., Wellington.
 UNIVERSITY OF NEW ZEALAND, Wellington.
 UNIVERSITY OF OTAGO, Dunedin.

British Borneo

SARAWAK MUSEUM, Kuching, Sarawak.

Indonesia

ARCHEOLOGICAL SERVICE OF INDONESIA, Jakarta.
 FACULTY OF ARTS, Jakarta.
 FACULTY OF LAW AND SOCIAL SCIENCES, Jakarta.
 INSTITUTE FOR INDONESIAN CULTURE, Jakarta.
 INSTITUTE FOR LINGUISTIC AND CULTURAL RESEARCH, Jakarta.
 UNIVERSITY OF INDONESIA, Jakarta.

Netherlands New Guinea

BUREAU FOR NATIVE AFFAIRS, Hollandia.

Philippines

MUSEUM AND INSTITUTE OF ARCHAEOLOGY AND ETHNOLOGY, Manila.
 NATIONAL MUSEUM, Manila.
 SILLIMAN UNIVERSITY, Dumaguete City.
 UNIVERSITY OF THE PHILIPPINES, Quezon City.

Singapore

MALAYAN BRANCH, ROYAL ASIATIC SOCIETY, Singapore.

RAFFLES MUSEUM AND LIBRARY, Singapore.
 UNIVERSITY OF MALAYA, Singapore.

Republic of China

INSTITUTE OF HISTORY AND PHILOLOGY, ACADEMIA SINICA, Yangmei.
 NATIONAL TAIWAN UNIVERSITY, Taipei.

Burma

ARCHAEOLOGICAL SURVEY OF BURMA, Rangoon.
 BURMA RESEARCH SOCIETY, Rangoon.
 UNIVERSITY OF RANGOON, Rangoon.

Cambodia

ALBERT SARRAUT MUSEUM, Phnom Penh.
 BUDDHIST INSTITUTE, Phnom Penh.
 HIGHER SCHOOL OF PALI, Phnom Penh.

Laos

VAT PRAH KEO MUSEUM AND VAT SISAKET ARCHEOLOGICAL COLLECTION, Vientiane.

Vietnam

BLANCHARD DE LA BROSE MUSEUM, Saigon.
 SOCIETY OF INDOCHINESE STUDIES, Saigon.

Thailand

NATIONAL MUSEUM, Bangkok.
 THE SIAM SOCIETY, Bangkok.

Japan

ABASHIRI MUSEUM, Abashiri City.
 AICHI UNIVERSITY, Toyohashi City.
 AOYAMA GAKUIN COLLEGE, Tokyo.
 ARCHEOLOGICAL INSTITUTE OF JAPAN, Ichi-kawa City.
 ATTIC MUSEUM INSTITUTE FOR JAPANESE FOLK CULTURE, Tokyo.
 DOSHISHA UNIVERSITY, Kyoto City.
 GUNMA UNIVERSITY, Maebashi City.
 HIROSHIMA MEDICAL COLLEGE, Kure City.
 HIROSHIMA UNIVERSITY, Hiroshima City.
 HOKKAIDO LIBERAL ARTS UNIVERSITY, Sapporo City.
 HOKKAIDO UNIVERSITY MUSEUM, Sapporo City.
 HOSIE UNIVERSITY, Tokyo.
 IBARAKI UNIVERSITY, Watari-mura.
 IBUN SOCIETY, Tsukuba City.
 IWATE MEDICAL COLLEGE, Morioka City.
 JUNTENDO UNIVERSITY, Tokyo.
 KAGOSHIMA UNIVERSITY, Kagoshima City.
 KANAZAWA UNIVERSITY, Kanazawa City.
 KANZAI GAKUIN UNIVERSITY, Nishinomiya City.
 KASHIHARA ARCHEOLOGICAL INSTITUTE, Ta-kaichi City.
 KEIO UNIVERSITY, Tokyo.
 KIBI ARCHEOLOGICAL MUSEUM, Tsukuba City.
 KOKUGAKUIN UNIVERSITY, Tokyo.
 ARCHEOLOGICAL SOCIETY OF JAPAN, Tokyo.
 JAPAN MEDICAL SCHOOL, Tokyo.
 JAPANESE ARCHEOLOGICAL ASSOCIATION, Tokyo.
 JAPANESE FOLKLORE SOCIETY, Tokyo.
 FAR EASTERN ARCHEOLOGICAL SOCIETY, Tokyo.
 JAPANESE SOCIETY OF ETHNOLOGY, Tokyo.
 JAPAN UNIVERSITY, Tokyo.
 KUMAMOTO UNIVERSITY, Kumamoto City.
 KUNITACHI MUSICAL COLLEGE, Tokyo.

KURASHIKI ARCHEOLOGICAL MUSEUM, Kurashiki City.
 KUSHIRO MUNICIPAL MUSEUM, Kushiro City.
 KYOTO UNIVERSITY, Kyoto City.
 KYOTO NATIONAL MUSEUM, Kyoto City.
 LINGUISTIC SOCIETY OF JAPAN, Tokyo.
 MATSUMOTO MUNICIPAL MUSEUM, Matsumoto City.
 MIYAZAKI PREFECTURAL MUSEUM, Miyazaki City.
 MUSASHINO MUSEUM, Tokyo.
 MUSEUM OF CALLIGRAPHY, Tokyo.
 NAGAOKA SCIENCE MUSEUM, Nagaoka City.
 NAGOYA UNIVERSITY, Nagoya City.
 ANTHROPOLOGICAL INSTITUTE OF NANZAN UNIVERSITY, Nagoya.
 ANTHROPOLOGICAL INSTITUTE, FACULTY OF SCIENCE, TOKYO UNIVERSITY, Tokyo.
 ANTHROPOLOGICAL SOCIETY OF JAPAN, Tokyo.
 INSTITUTE FOR ORIENTAL CULTURE OF TOKYO UNIVERSITY, Tokyo.
 HAKODATE CITY MUSEUM, Hakodate City.
 FOLKLORE INSTITUTE OF JAPAN, Tokyo.
 JAPANESE MUSEUM OF FOLK CRAFTS, Tokyo.
 JAPANESE MUSEUM OF ETHNOLOGY, Tokyo.
 CATHOLIC UNIVERSITY OF NAGOYA, Nagoya City.
 NATIONAL COMMITTEE OF ANTHROPOLOGY AND ETHNOLOGY, SCIENCE COUNCIL OF JAPAN, Tokyo.
 KYUSHU UNIVERSITY, Fukuoka City.
 MEIJI UNIVERSITY, Tokyo.
 NARA MEDICAL COLLEGE, Nara City.
 NIIGATA UNIVERSITY, Niigata City.
 MUSEUM OF THE NORTHERN CULTURE, Niigata City.
 NATIONAL MUSEUM OF TOKYO, Tokyo.
 NATIONAL MUSEUM, NARA BRANCH, Nara City.
 OCHANOMIZU UNIVERSITY FOR WOMEN, Tokyo.
 NATIONAL MUSEUM OF KYOTO, Kyoto City.
 OKAYAMA UNIVERSITY, Okayama City.
 OSAKA CITY UNIVERSITY, Osaka City.
 OSAKA UNIVERSITY, Osaka City.
 OSAKA UNIVERSITY OF ECONOMICS, Osaka City.
 OSAKA UNIVERSITY OF FOREIGN STUDIES, Osaka City.
 OSAKA FINE ARTS MUSEUM, Osaka City.
 OTANI UNIVERSITY, Kyoto City.
 THE ORIENTAL FOLK MUSEUM, Nara City.
 RESEARCH INSTITUTE FOR NATURAL RESOURCES, Tokyo.
 RESEARCH INSTITUTE OF HUMANISTIC SCIENCES, KYOTO UNIVERSITY, Kyoto City.
 PALAEOLOGICAL ASSOCIATION, Osaka City.
 SAITAMA UNIVERSITY, Urawa City.
 SOCIETY OF NATURAL AND CULTURAL HISTORY, Kyoto.
 SHINSHU UNIVERSITY, Shinshu City.
 SHIMANE UNIVERSITY, Matsue City.
 ST. PAUL'S RIKKYO UNIVERSITY, Tokyo.
 RITSUMEIKAN UNIVERSITY, Kyoto City.
 RYOKOKU UNIVERSITY, Kyoto City.
 SEIZAN LIBRARY, Kochi City.
 TENRI MUSEUM, Nara City.
 TOHOKU UNIVERSITY, Sendai City.
 TOKAI UNIVERSITY, Shimizu City.
 TOKYO METROPOLITAN UNIVERSITY, Tokyo.
 TOKYO UNIVERSITY, Tokyo.
 TOKYO UNIVERSITY OF FOREIGN STUDIES, Tokyo.
 TOKYO UNIVERSITY OF ARTS, Tokyo.
 TOKYO KYOIKU UNIVERSITY, Tokyo.
 TOKYO JIKIKAI MEDICAL COLLEGE, Tokyo.

TOKYO WOMENS CHRISTIAN COLLEGE, Tokyo.

TOYAMA UNIVERSITY, Toyama City.

TOYO UNIVERSITY, Tokyo.

UNIVERSITY OF LIBERAL ARTS IN TOKYO, Tokyo.

WASEDA UNIVERSITY, Tokyo.

YAMATO HISTORICAL MUSEUM, Nara City.

YOKOHAMA CITY UNIVERSITY, Yokohama City.

YOKOHAMA NATIONAL UNIVERSITY, Yokohama City.

Ceylon

DEPARTMENT OF NATIONAL MUSEUMS, Colombo.

India

AGRA UNIVERSITY, Agra, Uttar Pradesh.

ANTHROPOLOGICAL SOCIETY OF BOMBAY, Bombay.

ANTHROPOS INSTITUTE, INDIAN BRANCH, Bombay.

ARCHAEOLOGICAL MUSEUM MATHURA, Mathura, Uttar Pradesh.

ASSAM RESEARCH SOCIETY, Gauhati, Assam.

BANGABASI COLLEGE, Calcutta.

BARODA MUSEUM AND PICTURE GALLERY, Baroda.

BIHAR RESEARCH SOCIETY, Patna.

COTTON COLLEGE, Gauhati, Assam.

DECCAN COLLEGE, Poona.

DEPARTMENT OF ANTHROPOLOGY, GOVERNMENT OF INDIA, Calcutta.

DEPARTMENT OF ARCHAEOLOGICAL SURVEY OF INDIA, New Delhi.

DEPARTMENT OF ETHNOGRAPHY, GOVERNMENT OF ASSAM, Shillong, Assam.

ETHNOGRAPHIC AND FOLK CULTURE SOCIETY, UTTAR PRADESH, Lucknow, Uttar Pradesh.

GOVERNMENT MUSEUM, MADRAS, Madras, South India.

GUJARAT RESEARCH SOCIETY, Bombay.

INDIAN SCIENCE CONGRESS ASSOCIATION, Calcutta.

THE MAHARAJA SIYAJIRAO UNIVERSITY OF BARODA, Baroda.

THE MYTHIC SOCIETY, Bangalore.

NAGPUR UNIVERSITY, Nagpur.

NATIONAL MUSEUM OF INDIA, New Delhi.

OSMANIA UNIVERSITY, Hyderabad.

PATNA UNIVERSITY, Patna.

RAJPUTANA UNIVERSITY, Ajmer.

ROYAL ASIATIC SOCIETY OF BENGAL, Calcutta.

ROYAL ASIATIC SOCIETY, BOMBAY BRANCH, Bombay.

SOCIETY OF THE SERVANTS OF PRIMITIVE TRIBES IN INDIA, Delhi.

TAJ MUSEUM, Agra.

UNIVERSITY OF BOMBAY, Bombay.

UNIVERSITY OF CALCUTTA, Calcutta.

UNIVERSITY OF DELHI, Delhi.

UNIVERSITY OF GAUHATI, Gauhati, Assam.

UNIVERSITY OF LUCKNOW, Lucknow, Uttar Pradesh.

UNIVERSITY OF MADRAS, Madras.

Pakistan

ARCHAEOLOGICAL MUSEUM, HARAPPA, Dist. Montgomery, West Punjab.

ARCHAEOLOGICAL MUSEUM, MOHHENJODARO, Dohri, Dist. Larkana, Sind.

ARCHAEOLOGICAL MUSEUM, TAXILA, Rawalpindi.

NATIONAL MUSEUM OF PAKISTAN, Karachi.

VARENDRA RESEARCH SOCIETY AND MUSEUM, Rajshahi, East Bengal.

Afghanistan

FRENCH ARCHAEOLOGICAL DELEGATION IN AFGHANISTAN, Kabul.

KABUL MUSEUM, Kabul.

Iran

ARCHAEOLOGICAL MUSEUM, Tehran.

UNIVERSITY OF TEHRAN, Tehran.

Iraq

AMERICAN SCHOOL OF ORIENTAL RESEARCH, Baghdad.

BRITISH SCHOOL OF ARCHAEOLOGY IN IRAQ, Baghdad.

COLLEGE OF ARTS AND SCIENCE, Baghdad.

DIRECTORATE-GENERAL OF ANTIQUITIES, Baghdad.

Israel

AMERICAN SCHOOL OF ORIENTAL RESEARCH, Jerusalem.

BRITISH SCHOOL OF ARCHAEOLOGY AT JERUSALEM, Jerusalem.

COUNCIL OF THE HEBREW LANGUAGE, Jerusalem.

FOLKLORE SOCIETY IN ISRAEL, Tel Aviv.

THE HEBREW UNIVERSITY, Jerusalem.

ISRAEL DEPARTMENT OF ANTIQUITIES, Jerusalem.

ISRAEL EXPLORATION SOCIETY, Jerusalem.

ISRAEL INSTITUTE OF APPLIED SOCIAL RESEARCH, Jerusalem.

RESEARCH COUNCIL OF ISRAEL, Jerusalem.

Lebanon

AMERICAN UNIVERSITY OF BEIRUT, Beirut.

CENTRE D'ÉTUDES GÉOGRAPHIQUES DU PROCHE ET MOYEN ORIENT, Beirut.

INSTITUT FRANÇAIS D'ARCHÉOLOGIE DE BEYROUTH, Beirut.

MUSEUM OF THE AMERICAN UNIVERSITY OF BEIRUT, Beirut.

NATIONAL MUSEUM OF BEIRUT, Beirut.

UNIVERSITÉ ST. JOSEPH DE BEYROUTH, Beirut.

UNIVERSITÉ ST. JOSEPH DE BEYROUTH, Beirut.

UNIVERSITÉ ST. JOSEPH DE BEYROUTH, Beirut.

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UNIVERSITÉ ST. JOSEPH DE BEYROUTH, Beirut.

UNIVERSITÉ ST. JOSEPH DE BEYROUTH, Beirut.

Angola

MUSEU DE ANGOLA, Luanda.

MUSEU DO DUNDO, Luanda.

Ghana

UNIVERSITY COLLEGE OF GHANA, Achimota.

Kenya

CORYNDON MUSEUM, Nairobi.

CONSEIL SCIENTIFIQUE POUR L'AFRIQUE AU SUD DU SAHARA (CSA), Kikuyu.

Mozambique

SOCIEDADE DES ESTUDOS DE MOÇAMBIQUE, Lorenzo Marques.

Nigeria

UNIVERSITY COLLEGE, IBADAN, Ibadan.

WEST AFRICAN INSTITUTE OF SOCIAL AND ECONOMIC RESEARCH, Ibadan.

Northern Rhodesia

RHODES-LIVINGSTONE INSTITUTE, Lusaka.

RHODES-LIVINGSTONE MUSEUM, Livingstone.

Portuguese Guinea

CENTRO DE ESTUDOS DA GUINE PORTUGUESA, Bissau.

Senegal

INSTITUTE FRANÇAIS D'AFRIQUE NOIRE (IFAN), Dakar.

Sierra Leone

FOURAH BAY COLLEGE, Freetown.

Southern Rhodesia

NATIONAL MUSEUM OF SOUTHERN RHODESIA, Bulawayo.

QUEEN VICTORIA MEMORIAL MUSEUM, Salisbury.

Tanganyika

KING GEORGE V MEMORIAL MUSEUM, Dar-es-Salaam.

Uganda

EAST AFRICAN INSTITUTE OF SOCIAL RESEARCH, Kampala.

MAKERERE COLLEGE, THE UNIVERSITY COLLEGE OF EAST AFRICA, Kampala.

UGANDA MUSEUM, Kampala.

Union of South Africa

AFRICAN MUSIC SOCIETY, Johannesburg, Transvaal.

AFRICANA MUSEUM, Johannesburg, Transvaal.

ARCHAEOLOGICAL SURVEY OF THE UNION OF SOUTH AFRICA, Johannesburg, Transvaal.

DURBAN MUSEUM, Durban, Natal.

NATIONAL MUSEUM, Bloemfontein, Orange Free State.

RHODES UNIVERSITY, Grahamstown, Cape Province.

SOUTH AFRICAN ARCHAEOLOGICAL SOCIETY, Claremont, Cape Province.

SOUTH AFRICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, Johannesburg, Transvaal.

SOUTH AFRICAN INSTITUTE OF RACE RELATIONS, Johannesburg, Transvaal.

SOUTH AFRICAN MUSEUM, Cape Town, Cape Province.

TRANSVAAL MUSEUM, Pretoria, Transvaal.

UNIVERSITY OF CAPE TOWN, Rondebosch, Cape Province.

UNIVERSITY OF NATAL, Durban and Pietermaritzburg, Natal.
UNIVERSITY OF PRETORIA, Pretoria, Transvaal.
UNIVERSITY OF STELLENBOSCH, Stellenbosch, Cape Province.
UNIVERSITY OF THE WITWATERSRAND, Johannesburg, Transvaal.

Zanzibar

PEACE MUSEUM, Zanzibar.

Algeria

INSTITUT DE RECHERCHES SAHARIENNES DE L'UNIVERSITÉ D'ALGER, Algiers.
MUSÉE D'ETHNOGRAPHIE ET DE PRÉHISTOIRE DU BARDO, Algiers.
UNIVERSITÉ D'ALGER, Algiers.

Egypt

ANATOMY MUSEUM, KASR EL AINI MEDICAL COLLEGE, Cairo.
COPTIC MUSEUM, Old Cairo, Cairo.
FOUAD I DESERT INSTITUTE, Heliopolis.
INSTITUT D'EGYPT, Cairo.
INSTITUT FRANÇAIS D'ARCHÉOLOGIE ORIENTALE DU CAIRE, Cairo.
INSTITUTE OF SUDANESE STUDIES, FOUAD I UNIVERSITY, Zamalek, Cairo.
MUSEUM OF EGYPTIAN ANTIQUITIES, Cairo.
SERVICE DES ANTIQUITÉS DE L'EGYPTE, Cairo.
SOCIÉTÉ ROYALE DE GÉOGRAPHIE D'EGYPT, Cairo.

Morocco

INSTITUT DE PSYCHOLOGIE ET DE SOCIOLOGIE APPLIQUÉES, Casablanca.
INSTITUT DES HAUTES ÉTUDES MAROCAINES, Rabat.
MUSÉE DE L'INSPECTION DES ANTIQUITÉS, Rabat.
SERVICE DES MÉTIERS ET ARTS MAROCAINS, Rabat.
SOCIÉTÉ D'ÉTUDES ÉCONOMIQUES, SOCIALES ET STATISTIQUES, Rabat.
SOCIÉTÉ DE GÉOGRAPHIE DU MAROC, Rabat.
SOCIÉTÉ DE PRÉHISTOIRE DU MAROC, Casablanca.

Sudan

KHARTOUM MUSEUM, Khartoum.

Tunisia

DIRECTION DES ANTIQUITÉS ET ARTS, Tunis.
INSTITUT DES BELLES LETTRES ARABES, Tunis.
INSTITUT DES HAUTES ÉTUDES DE TUNIS, Tunis.
MUSÉE ALAOUÏ, Le Bardo.
OFFICE DES ARTS TUNISIENS, Tunis.

Greece

ANTHROPOLOGICAL MUSEUM, Athens.
AMERICAN SCHOOL OF CLASSICAL STUDIES AT ATHENS, Athens.
ARCHAEOLOGICAL MUSEUM OF THESSALONIKI, Salonika.
ARCHAEOLOGICAL SOCIETY, Athens.
ATHENS NATIONAL UNIVERSITY, Athens.
BENAKI MUSEUM, Athens.
BRITISH SCHOOL OF ARCHAEOLOGY AT ATHENS, Athens.
FOLKLORE ARCHIVE AND MUSEUM, Salonika.
FOLKLORE ARCHIVE OF THE ACADEMY OF ATHENS, Athens.
ÉCOLE FRANÇAISE D'ATHÈNES, Athens.
GREEK FOLKLORE SOCIETY, Athens.
HELLENIC ANTHROPOLOGICAL SOCIETY, Athens.

LABORATORY OF EXPERIMENTAL PAEDAGOGY, ATHENS NATIONAL UNIVERSITY, Athens.
LINGUISTIC SOCIETY, Athens.
NATIONAL ARCHAEOLOGICAL MUSEUM, Athens.
NATIONAL MUSEUM OF DECORATIVE ART, Athens.
PANDEIOS HIGHER SCHOOL OF POLITICAL SCIENCES, Athens.
PSYCHOLOGICAL LABORATORY, ATHENS NATIONAL UNIVERSITY, Athens.
SOCIETY OF MACEDONIAN STUDIES, Salonika.
UNIVERSITY OF THESSALONIKI, Salonika.

Italy

ACCADEMIA NAZIONALE DEI LINCEI, Rome.
ASSOCIAZIONE ITALIANA DI PSICOLOGIA.
BRITISH SCHOOL AT ROME, Rome.
CENTRO DI PSICOLOGIA APPLICATA, Venice.
COMITATO DI STUDI PREISTORICI NELL'EMILIA OCCIDENTALE, Parma.
COMITATO INTERNAZIONALE PER LA UNIFICAZIONE DEI METODI E PER LA SINTESI IN ANTROPOLOGIA, EUGENICA, E BIOLOGIA, Bologna.
COMITATO ITALIANO PER LO STUDIO DEI PROBLEMI DELLA POPOLAZIONE, Rome.
ISTITUTO DE STUDI ETRUSCHI ED ITALICI, Florence.
ISTITUTO DI STUDI SARDI, Cagliari.
ISTITUTO ITALIANO DI ANTROPOLOGIA, Rome.
ISTITUTO NAZIONALE DI ARCHEOLOGIA E STORIA DELL'ARTE, Rome.
ISTITUTO SUPERIORE DI MAGISTERO, Genoa.
ISTITUTO SUPERIORE DI MAGISTERO PAREGGIATO, Salerno.
ISTITUTO UNIVERSITARIO DI ECONOMIA E COMMERCIO, Venice.
ISTITUTO UNIVERSITARIO ORIENTALE DI NAPOLI, Naples.
ISTITUTO UNIVERSITARIO PAREGGIATO DI MAGISTERO FEMMINILE "SUOR ORSOLA BENINCASA," Naples.
ISTITUTO UNIVERSITARIO PAREGGIATO DI MAGISTERO "SANTA MARIA ASSUNTA," Rome.
MUSEO ARCHEOLOGICO, Aosta.
MUSEO ARCHEOLOGICO, Aquileia.
MUSEO ARCHEOLOGICO, Asti.
MUSEO ARCHEOLOGICO, Bari.
MUSEO ARCHEOLOGICO, Castiglione.
MUSEO ARCHEOLOGICO, Florence.
MUSEO ARCHEOLOGICO, Venice.
MUSEO ARCHEOLOGICO, Verona.
MUSEO ARCHEOLOGICO DI SPINA, Ferrara.
MUSEO ARCHEOLOGICO FEDERICO EUSEBIO, Alba.
MUSEO ARCHEOLOGICO GIROLAMO ROSSI, Ventimiglia.
MUSEO ARCHEOLOGICO LUNENSE, La Spezia.
MUSEO ARCHEOLOGICO MECENATE, Arezzo.
MUSEO ARCHEOLOGICO NAZIONALE, Cagliari.
MUSEO ARCHEOLOGICO NAZIONALE, Cividale del Friuli.
MUSEO ARCHEOLOGICO NAZIONALE, Syracuse.
MUSEO ARCHEOLOGICO POMPEO ARIA, Mazza-botto.
MUSEO CIVICO ARCHEOLOGICO, Agrigento.
MUSEO CIVICO DI ARCHEOLOGIA LIGURE, Genoa-Pegli.
MUSEO DI ANTICHITA, Turin.
MUSEO DI ETNOGRAFIA ITALIANA, Tivoli.
MUSEO EGIZIO, Turin.
MUSEO ETNOGRAFICO GIUSEPPE PITRE, Palermo.
MUSEO ETRUSCO, Chiusi.
MUSEO FIORENTINO DI PREISTORIA, Florence.
MUSEO LAPIDARIO ARCHEOLOGICO, Bergamo Alta.

MUSEO NAZIONALE DI ANTICHITA, Parma.
MUSEO NAZIONALE DI VILLA GIULIA, Rome.
RACCOLTA ARCHEOLOGICA, Amelia.
RACCOLTA ARCHEOLOGICA DON CESARE QUIROLI, Vado Ligure.
SOCIETÀ DI ETNOGRAFIA ITALIANA, Rome.
SOCIETÀ ITALIANA DI ANTROPOLOGIA E ETNOLOGIA, Florence.
SOCIETÀ ITALIANA DI SOCIOLOGIA, Rome.
UNIVERSITÀ CATTOLICA DEL SACRO CUORE, Milan.
UNIVERSITÀ DI BARI, Bari.
UNIVERSITÀ DI BOLOGNA, Bologna.
UNIVERSITÀ DI CAGLIARI, Cagliari.
UNIVERSITÀ DI CATANIA, Catania.
UNIVERSITÀ DI ECONOMIA E COMMERCIO "LUIGI BOCCONI," Milan.
UNIVERSITÀ DI FERRARA, Ferrara.
UNIVERSITÀ DI FIRENZE, Florence.
UNIVERSITÀ DI GENOVA, Genoa.
UNIVERSITÀ DI MESSINA, Messina.
UNIVERSITÀ DI MILANO, Milan.
UNIVERSITÀ DI NAPOLI, Naples.
UNIVERSITÀ DI PADOVA, Padua.
UNIVERSITÀ DI PALERMO, Palermo.
UNIVERSITÀ DI PARMA, Parma.
UNIVERSITÀ DI PAVIA, Pavia.
UNIVERSITÀ DI PISA, Pisa.
UNIVERSITÀ DI ROMA, Rome.
UNIVERSITÀ DI SIENA, Siena.
UNIVERSITÀ DI TORINO, Turin.
UNIVERSITÀ DI TRIESTE, Trieste.
UNIVERSITÀ LIBERA DI CAMERINO, Macerata.
UNIVERSITÀ LIBERA DI URBINO, Urbino.

Vatican City

MUSEO MISSIONARIO ETNOLOGICO, Rome.
PONTIFICIA ACCADEMIA ROMANA DI ARCHEOLOGIA, Rome.

Spain

ACADEMIA ALFONSO X EL SABIO, Murcia.
CENTRO DE CULTURA VALENCIANA, Lonja, Valencia.
CENTRO DE ESTUDIOS DE ETNOLOGÍA PENINSULAR, Madrid.
CENTRO DE ESTUDIOS DE ETNOLOGÍA PENINSULAR, Barcelona.
CENTRO DE ESTUDIOS MONTAÑESES, Santander.
CONSEJO SUPERIOR DE INVESTIGACIONES CIENTÍFICAS (CSIC), Madrid.
ESCUELA DE ESTUDIOS HISPANO-AMERICANOS, Seville.
INSTITUTO "BERNARDINO DE SAHAGÚN" (DE ANTROPOLOGÍA Y ETNOLOGÍA), Madrid.
INSTITUTO "RODRIGO CARO" DE ARQUEOLOGÍA, Madrid.
INSTITUTO "MIGUEL ASIN" DE ESTUDIOS ARABES, Madrid.
INSTITUTO "BENITO ARIAS MONTANO" DE ESTUDIOS HEBRAICOS Y ORIENTE PRÓXIMO, Madrid.
INSTITUTO DE PREHISTORIA MEDITERRÁNEA, Barcelona.
INSTITUTO ESPAÑOL DE MUSICOLOGÍA, Barcelona.
INSTITUTO "PADRE SARMIENTO" DE ESTUDIOS GALLEGOS, Santiago de Compostela.
INSTITUTO "JUAN SEBASTIÁN ELCAÑO" DE GEOGRAFÍA, Madrid.
INSTITUTO DE ESTUDIOS AFRICANOS, Madrid.
INSTITUTO DE ESTUDIOS PIRENAICOS, Zaragoza.
INSTITUTO BALMES DE SOCIOLOGÍA, Madrid.
INSTITUTO DE ESTUDIOS CANARIOS, La Laguna, Canary Islands.
INSTITUTO DE ESTUDIOS IBERDENSES, Lérida.
INSTITUTO FERNANDO EL CATÓLICO, Zaragoza.
INSTITUTO PRINCEPE DE VIANA, Pamplona.

INSTITUTO DE ESTUDIOS RIOJANOS, Logroño.
 INSTITUTO DE ESTUDIOS ASTURIANOS, Oviedo.
 INSTITUTO DE ESTUDIOS GERUNDENSES, Gerona.
 INSTITUTO ALFONSO EL MAGNANIMO, Valencia.
 INSTITUTO FERNÁN GONZÁLEZ, Burgos.
 INSTITUTO DE ESTUDIOS SEGOVIANOS DIEGO DE COLMENARES, Segovia.
 INSTITUTO DE ESTUDIOS MALAGUEÑOS, Málaga.
 INSTITUTO DE ESTUDIOS TIROLENSES, Teruel.
 INSTITUTO DE TELLO TÉLLEZ DE MENESES, Palencia.
 INSTITUTO DE ESTUDIOS MANCHEGOS, Ciudad Real.
 INSTITUTO DE ESTUDIOS IBICENCOS, Ibiza.
 JUNTA DE CULTURA DE VISCAYA, Bilbao.
 MUSEO ARQUEOLÓGICO, Barcelona.
 MUSEO ARQUEOLÓGICO Y ETNOGRÁFICO, Bilbao.
 MUSEO ARQUEOLÓGICO NACIONAL, Madrid.
 MUSEO CANARIO, Las Palmas, Canary Islands.
 MUSEO DE AMÉRICA, Madrid.
 MUSEO DE INDUSTRIAS Y ARTES POPULARES, Barcelona.
 MUSEO DE PONTEVEDRA, Pontevedra.
 MUSEO DE SAN TELMO, Guipúzcoa.
 MUSEO DEL PUEBLO ESPAÑOL, Madrid.
 MUSEO ETNOLÓGICO, Madrid.
 MUSEO ETNOLÓGICO, Barcelona.
 REAL ACADEMIA DE CIENCIAS, BELLAS LETRAS, Y NOBLES ARTES DE CÓRDOBA, Córdoba.
 REAL SOCIEDAD VASCONGADA DE AMIGOS DEL PAÍS, San Sebastian.
 SERVICIO DE INVESTIGACIÓN PREHISTÓRICA DE LA EXCM. DIPUTACIÓN DE VALENCIA (SIP), Valencia.
 SERVICIOS CULTURALES EXTREMEÑOS, Badajoz.
 SOCIEDAD CASTELLONENSE DE CULTURA, Castellón.
 SOCIEDAD ESPAÑOLA DE ANTROPOLOGÍA, ETNOGRAFÍA Y PREHISTORIA, Madrid.
 UNIVERSIDAD DE MADRID, Madrid.
 UNIVERSIDAD DE BARCELONA, Barcelona.

Portugal

ASSOCIAÇÃO DOS ARQUEÓLOGOS PORTUGUESES, Lisbon.
 CENTRO DE ESTUDIOS DE ETNOLOGIA PENINSULAR, Porto.
 COMISSÃO DE ETNOGRAFIA E HISTÓRIA Y MUSEU DE ETNOGRAFIA E HISTÓRIA DA PROVINCIA DO DOURO LITORAL, Porto.
 ESCOLA SUPERIOR COLONIAL, Lisbon.
 INSTITUTO PORTUGUÊS DE ARQUEOLOGIA, HISTÓRIA, E ETNOGRAFIA, Lisbon.
 LABORATORIO E MUSEU ZOOLOGICO E ANTROPOLÓGICO (MUSEU BOCAGE), UNIVERSIDADE DE LISBOA, Lisbon.
 MUSEU ARQUEOLÓGICO, Lisbon.
 MUSEU ETNOLÓGICO "DR. LEITE DE VASCONCELOS," Lisbon.
 MUSEU E LABORATÓRIO ANTROPOLÓGICO (INSTITUTO DE ANTROPOLOGIA), UNIVERSIDADE DE COIMBRA, Coimbra.
 MUSEU E LABORATÓRIO ANTROPOLÓGICO, UNIVERSIDADE DO PORTO, Porto.
 SOCIEDADE MARTINS SARMENTO, Guimarães.
 UNIVERSIDADE DE COIMBRA, Coimbra.
 UNIVERSIDADE DE LISBOA, Lisbon.
 UNIVERSIDADE DO PORTO, Porto.

France

UNIVERSITÉ DE PARIS À LA SORBONNE, Paris.
 Institut d'Ethnologie.

Faculté des Sciences.
 Faculté des Lettres.
 École Pratique des Hautes Études, Laboratoire d'Anthropologie Physique.
 École Pratique des Hautes Études, Section des Sciences Economiques et Sociales.
 Centre des Hautes Études d'Administration Musulmane.
 UNIVERSITÉ DE DIJON, Dijon.
 UNIVERSITÉ DE LYON, Lyon.
 UNIVERSITÉ DE POITIERS, Poitiers.
 UNIVERSITÉ DE RENNES, Rennes.
 UNIVERSITÉ DE TOULOUSE, Toulouse.
 CENTRE DE FORMATION AUX RECHERCHES ETHNOLOGIQUES (C.F.R.E.), Paris.
 COLLÈGE DE FRANCE, Paris.
 ÉCOLE D'ANTHROPOLOGIE, Paris.
 ÉCOLE NATIONALE DE LA FRANCE D'OUTRE-MER, Paris.
 INSTITUT CATHOLIQUE DE PARIS, Paris.
 INSTITUT NATIONAL D'ÉTUDES DÉMOGRAPHIQUES, Paris.
 MUSÉE DE L'HOMME—LABORATOIRE D'ETHNOLOGIE DES HOMMES ACTUELS ET DES HOMMES FOSSILES, Paris.
 INSTITUT DES ÉTUDES PRÉHISTORIQUES DES EYZIES, Les Eyzies.
 MUSÉE NATIONAL DES ARTS ET TRADITIONS POPULAIRES, Paris.
 MUSÉE DES ANTIQUITÉS NATIONALES, Saint-Germain-en-Laye.
 INSTITUT DE PALÉONTOLOGIE HUMAINE, Paris.
 CENTRE DE DOCUMENTATION ET DE RECHERCHES PRÉHISTORIQUES, Paris.
 MUSÉE DES SCIENCES NATURELLES DE LYON, Lyon.
 INSTITUT D'ANTHROPOLOGIE GÉNÉRALE, UNIVERSITÉ DE RENNES, Rennes.
 LABORATOIRE DU PEYRAT, Villebois-Lavalette.
 ASSOCIATION FRANÇAISE POUR L'AVANCEMENT DES SCIENCES, Paris.
 COMITÉ DU FILM ETHNOGRAPHIQUE, Paris.
 COMITÉ TECHNIQUE DE LA RECHERCHE ARCHÉOLOGIQUE EN FRANCE, CENTRE NATIONAL DE LA RECHERCHE SCIENTIFIQUE (CNRS), Paris.
 INSTITUT FRANÇAIS D'ANTHROPOLOGIE (IFA), Paris.
 SOCIÉTÉ DES AFRICANISTES, Paris.
 SOCIÉTÉ D'ANTHROPOLOGIE À PARIS, Paris.
 SOCIÉTÉ DES AMÉRICANISTES, Paris.
 SOCIÉTÉ ASIATIQUE, Paris.
 SOCIÉTÉ D'ETHNOGRAPHIE FRANÇAISE, Paris.
 SOCIÉTÉ DES OcéANISTES, Paris.
 SOCIÉTÉ PRÉHISTORIQUE FRANÇAISE, Paris.
 CENTRE NATIONAL DE LA RECHERCHE SCIENTIFIQUE (CNRS), Paris.
 OFFICE DE LA RECHERCHE SCIENTIFIQUE OUTRE-MER (ORSOM), Paris.
 COMMISSION SUPÉRIEURE DES MONUMENTS HISTORIQUES ET COMITÉ DES FOUILLES, Paris.
 COMMISSION DES FOUILLES ARCHÉOLOGIQUES, Paris.

The Netherlands

AFRIKA-INSTITUUT, Leiden.
 LANDBOUWHOGESCHOOL, Wageningen.
 RIJKSUNIVERSITEIT-GRONINGEN, Groningen.
 RIJKSUNIVERSITEIT-LEIDEN, Leiden.
 RIJKSUNIVERSITEIT-UTRECHT, Utrecht.
 ROOMS-KATHOLIEKE LEERGANGEN, Tilburg.
 ROOMS-KATHOLIEKE UNIVERSITEIT, Nijmegen.
 UNIVERSITEIT VAN AMSTERDAM, Amsterdam.
 KONINKLIJK INSTITUUT VOOR DE TROPEN, Amsterdam.
 MUSEUM VOOR HET ONDERWIJS, The Hague.

MUSEUM VOOR LAND- EN VOLKENKUNDE, Rotterdam.
 RIJKSMUSEUM VOOR VOLKENKUNDE, Leiden.
 KONINKLIJK INSTITUUT VOOR TAAL-, LAND- EN VOLKENKUNDE, The Hague.
 NEDERLANDSE ETHNOLOGENKRING, Utrecht.
 NEDERLANDS GENOOTSCHAP VOOR ANTHROPOLOGIE, Amsterdam.
 NEDERLANDSE ORGANISATIE VOOR ZUIVERWETENSCHAPPELIJK ONDERZOEK, The Hague.
 STICHTING VOOR DE CULTURELE SAMENWERKING, Amsterdam.

Belgium

INSTITUT UNIVERSITAIRE DES TERRITOIRES D'OUTRE-MER, Antwerp.
 RIJKSUNIVERSITEIT-GENT, Ghent.
 UNIVERSITÉ CATHOLIQUE, Louvain.
 UNIVERSITÉ DE L'ÉTAT DE LIÈGE, Liège.
 UNIVERSITÉ LIBRE DE BRUXELLES, Brussels.
 AFRICAN ART RESEARCH CENTER, STATE UNIVERSITY-GHENT, Tervuren.
 INSTITUT DE SOCIOLOGIE SOLVAY, Brussels.
 MUSÉE DU CONGO BELGE, Tervuren.
 MUSÉES ROYAUX D'ART ET D'HISTOIRE, Brussels.
 ETHNOGRAFISCH MUSEUM, Antwerp.
 FONDS NATIONAL DE LA RECHERCHE SCIENTIFIQUE, Brussels.
 INSTITUT POUR LA RECHERCHE SCIENTIFIQUE EN AFRIQUE CENTRALE (IRSAC), Brussels.
 INSTITUT ROYAL COLONIAL BELGE, Brussels.

Great Britain

UNIVERSITY OF LONDON, London.
 University College London, London.
 London School of Economics and Political Science, London.
 Institute of Archaeology, London.
 School of Oriental and African Studies, London.
 Birkbeck College, London.
 St. Thomas' Hospital Medical School, London.
 UNIVERSITY OF CAMBRIDGE, Cambridge.
 UNIVERSITY OF OXFORD, Oxford.
 Institute of Social Anthropology, Oxford.
 Department of Human Anatomy, Oxford.
 UNIVERSITY OF MANCHESTER, Manchester.
 UNIVERSITY OF BIRMINGHAM, Birmingham.
 UNIVERSITY OF LEEDS, Leeds.
 UNIVERSITY OF DURHAM, Durham.
 HUNTERIAN MUSEUM AND MUSEUM OF HUMAN AND COMPARATIVE ANATOMY, London.
 BRITISH MUSEUM, London.
 BRITISH MUSEUM (NATURAL HISTORY), London.
 HORNIMAN MUSEUM AND LIBRARY, London.
 PITT RIVERS MUSEUM, Oxford.
 ASHMolean MUSEUM, Oxford.
 UNIVERSITY MUSEUM OF ARCHAEOLOGY AND ETHNOLOGY, Cambridge.
 THE MANCHESTER MUSEUM, Manchester.
 NUFFIELD BLOOD GROUP CENTRE, ROYAL ANTHROPOLOGICAL INSTITUTE, London.
 BLOOD GROUP REFERENCE LABORATORY, LISTER INSTITUTE, London.
 ROYAL ANTHROPOLOGICAL INSTITUTE OF GREAT BRITAIN AND IRELAND, London.
 ASSOCIATION OF SOCIAL ANTHROPOLOGISTS.
 BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, London.
 THE FOLK-LORE SOCIETY, London.
 BRITISH SOCIOLOGICAL ASSOCIATION, London.
 COUNCIL FOR BRITISH ARCHAEOLOGY (C.B.A.), London.

SOCIETY OF ANTIQUARIES OF LONDON, London.

ROYAL ARCHAEOLOGICAL INSTITUTE OF GREAT BRITAIN AND IRELAND, London.

BRITISH ARCHAEOLOGICAL ASSOCIATION, London.

THE PREHISTORIC SOCIETY.

COLONIAL SOCIAL SCIENCE RESEARCH COUNCIL, London.

COLONIAL MEDICAL RESEARCH COUNCIL, London.

EMSLIE HORNIMAN SCHOLARSHIP TRUST FUND, London.

UNIVERSITY OF EDINBURGH, Edinburgh.

UNIVERSITY OF GLASGOW, Glasgow.

NATIONAL MUSEUM OF ANTIQUITIES OF SCOTLAND, Edinburgh.

ROYAL SCOTTISH MUSEUM, Edinburgh.

UNIVERSITY OF WALES, Cardiff.

University College of Wales, Aberystwyth.

NATIONAL MUSEUM OF WALES, Cardiff.

NATIONAL MUSEUM OF WALES (WELSH FOLK MUSEUM), S. Fagans.

Eire

NATIONAL UNIVERSITY OF IRELAND, Dublin.

University College, Dublin.

University College, Cork.

University College, Galway.

TRINITY COLLEGE, Dublin.

COIMISIUN BEALOIDEASA ÉIREANN, Dublin.

NATIONAL MUSEUM OF IRELAND, Dublin.

ROYAL SOCIETY OF ANTIQUARIES OF IRELAND, Dublin.

ROYAL IRISH ACADEMY, Dublin.

Denmark

AARHUS UNIVERSITET, Aarhus.

KØBENHAVNS UNIVERSITET, Copenhagen.

AALBORG HISTORISKE MUSEUM, Aalborg.

DANSK FOLKEMINDESAMLING, Copenhagen.

FORHISTORISK MUSEUM-AARHUS UNIVERSITETS FORHISTORISK ARKÆOLOGISKE INSTITUT, Aarhus.

FØROYA FORNGRIPASAVN, Tórshavn.

HADERSLEV AMTS MUSEUM, Haderslev.

DEN KLASSISK-ARKÆOLOGISKE STUDIESAMLING, AARHUS UNIVERSITET, Aarhus.

MUSEET FOR THY OG VESTRE HAN HERRED, Thisted.

MUSEET PAA SØNDERBORG SLOT, Sønderborg.

NATIONALMUSEET, Copenhagen.

ODENSE BYS MUSEER, Odense.

UDVALG FOR FOLKEMAALE, Copenhagen.

UNIVERSITETENS ANTHROPOLOGISKE LABORATORIUM, Copenhagen.

DANSK ETNOGRAFISK FORENING, Copenhagen.

DET KONGELIGE NORDISKE OLDSKRIFT SELSKAB, Copenhagen.

JYSK ARKÆOLOGISK SELSKAB, Aarhus.

CARLSBERGFONDET, Copenhagen.

DANSK EKSPEDITIONSFOND, Copenhagen.

RASK-ØRSTED FONDET, Copenhagen.

KNUD RASMUSSEN FONDET, Copenhagen.

STATENS ALMINDELIGE VIDENSKABSFOND, Copenhagen.

Finland

ÅBO AKADEMI, Turku.

HELSINGIN YLIOPISTO—HELSINGFORS UNIVERSITET, Helsinki.

TURUN YLIOPISTO, Turku.

INSTITUTET FÖR NORDISK ETNOLOGI VID ÅBO AKADEMI, Turku.

KANSALLISMUSEO—FINLANDS NATIONALMUSEUM, Helsinki.

SANAKIRJASÄÄTIÖ, Helsinki.

Sweden

GÖTEBORGS HÖGSKOLA, Göteborg.

KUNGLIGA UNIVERSITETET I LUND, Lund.

Stockholms Högskola, Stockholm.

KUNGLIGA UNIVERSITETET I UPPSALA, Uppsala.

BORÅS MUSEUM, Borås.

ETNOGRAFISKA MUSEET, Göteborg.

FOLKLIVSARKIVET, Lund.

GÖTEBORGS HISTORISKA MUSEUM, Göteborg.

GÖTLANDS FORNSAL, Visby.

INSTITUTET FÖR FOLKLIVSFORSKNING, Stockholm.

INSTITUTET FÖR ORTNAMNS- OCH DIALEKT-FORSKNING VID GÖTEBORGS HÖGSKOLA, Göteborg.

INSTITUTET FÖR VÄSTSVENSK KULTURFORSKNING VID GÖTEBORGS HÖGSKOLA, Göteborg.

JÄMTLANDS LÄNS MUSEUM, Östersund.

JÖNKÖPING LÄNS HEMBYGDSFÖRBUND, Jönköping.

KALMAR LÄNS MUSEUM, Kalmar.

KYRKOHISTORISKA ARKIVET I LUND, Lund.

LANDSMÅLSARKIVET I LUND, Lund.

LANDSMÅLS- OCH FOLKMINNESARKIVET, Uppsala.

LUNDS UNIVERSITETS ANATOMISKA INSTITUT, Lund.

LUNDS UNIVERSITETS HISTORISKA MUSEUM, Lund.

MALMÖ MUSEUM, Malmö.

NORDISKA MUSEET, Stockholm.

NORRBOTTENS MUSEUM, Luleå.

ÖREBRO LÄNS MUSEUM, Örebro.

ÖSTASIATISKA SAMLINGARNA, Stockholm.

RIKSANTIKVARIÄMBETET OCH STATENS HISTORISKA MUSEUM, Stockholm.

STATENS ETNOGRAFISKA MUSEUM, Stockholm.

STATENS INSTITUT FÖR RASBIOLOGI, Uppsala.

VÄRBERGS MUSEUM, Värberg.

VÄRMLANDS MUSEUM, Karlstad.

VÄSTERBOTTENS LÄNS MUSEUM, Umeå.

VÄSTSVENSKA FOLKMINNESARKIVET, Göteborg.

KULTURHISTORISKA FÖRENINGEN FÖR SÖDRA SVERIGE VID KULTURHISTORISKA MUSEET, Lund.

KUNGLIGA GUSTAF ADOLF AKADEMIEFÖR FOLKLIVSFORSKNING, Stockholm.

KUNGLIGA VETTERHETS HISTORIE OCH ANTIK-VITETS AKADEMIEFÖR, Stockholm.

SVENSKA SÄLSKAPET FÖR ANTHROPOLOGI OCH GEOGRAFI, Stockholm.

HUMANISTISKA FONDEN, Stockholm.

LÄNGMANSKA KULTURFONDEN, Uppsala.

Germany

FRIE UNIVERSITÄT BERLIN, Berlin-Dahlem.

RHEINISCHE FRIEDRICH-WILHELMS-UNIVERSITÄT BONN, Bonn.

JOHANN WOLFGANG GOETHE UNIVERSITÄT FRANKFURT AM MAIN, Frankfurt-on-Main.

GEORG AUGUST-UNIVERSITÄT ZU GÖTTINGEN, Göttingen.

UNIVERSITÄT HAMBURG, Hamburg.

CHRISTIAN-ALBRECHTS-UNIVERSITÄT KIEL, Kiel.

UNIVERSITÄT KÖLN, Cologne.

JOHANNES GUTENBERG UNIVERSITÄT MAINZ, Mainz.

LUDWIG-MAXIMILIANS-UNIVERSITÄT MÜNCHEN, Munich.

WESTFÄLISCHE-LANDES-UNIVERSITÄT MÜNSTER, Münster.

EBERHARD-KARLS-UNIVERSITÄT TÜBINGEN, Tübingen.

EHEMALIGES STAATLICHES MUSEUM FÜR VÖLKERKUNDE, Berlin-Dahlem.

FROBENIUS-INSTITUT AN DER JOHANN WOLFGANG GOETHE UNIVERSITÄT FRANKFURT AM MAIN, Frankfurt-on-Main.

Stockholms Högskola, Stockholm.

KUNGLIGA UNIVERSITETET I UPPSALA, Uppsala.

BORÅS MUSEUM, Borås.

ETNOGRAFISKA MUSEET, Göteborg.

FOLKLIVSARKIVET, Lund.

GÖTEBORGS HISTORISKA MUSEUM, Göteborg.

GÖTLANDS FORNSAL, Visby.

INSTITUTET FÖR FOLKLIVSFORSKNING, Stockholm.

INSTITUTET FÖR ORTNAMNS- OCH DIALEKT-FORSKNING VID GÖTEBORGS HÖGSKOLA, Göteborg.

INSTITUTET FÖR VÄSTSVENSK KULTURFORSKNING VID GÖTEBORGS HÖGSKOLA, Göteborg.

JÄMTLANDS LÄNS MUSEUM, Östersund.

JÖNKÖPING LÄNS HEMBYGDSFÖRBUND, Jönköping.

KALMAR LÄNS MUSEUM, Kalmar.

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SOCIEDAD MEXICANA DE GEOGRAFÍA Y ESTADÍSTICA, Mexico, D.F.

SOCIEDAD DE ALUMNOS DE LA ESCUELA NACIONAL DE ANTHROPOLOGÍA E HISTORIA, Mexico, D.F.

INSTITUTO LINGÜÍSTICO DE VERANO, Mexico, D.F.

CONGRESO MEXICANO DE HISTORIA, Mexico, D.F.

SOCIEDAD FOLKLORICA DE MEXICO, Mexico, D.F.

AGRUPACION FOLKLORICA MEXICANA, Mexico 1, D.F.

GRUPO CULTURAL ESPIRITU DE PUEBLA, Puebla.

Guatemala

UNIVERSIDAD DE SAN CARLOS DE GUATEMALA, Guatemala.

SAN CARLOS SUMMER SCHOOL, Guatemala.

INSTITUTO DE ANTHROPOLOGÍA E HISTORIA, Guatemala.

MUSEO NACIONAL DE ARQUEOLOGÍA Y ETNOLOGÍA, Guatemala.

INSTITUTO INDIGENISTA NACIONAL DE GUATEMALA, Guatemala.

INSTITUTE OF NUTRITION OF CENTRAL AMERICA AND PANAMA, Guatemala.

MUSEO ARQUEOLÓGICO DE CHICHICASTENANGO, Chichicastenango.

MUSEO ARQUEOLÓGICO DE ZACULEU, Huehuetenango.

MUSEO DE SAN MARCOS, San Marcos.

SOCIEDAD DE GEOGRAFÍA E HISTORIA DE GUATEMALA, Guatemala.

ASOCIACIÓN DE COLABORADORES DEL INSTITUTO DE ANTHROPOLOGÍA E HISTORIA DE GUATEMALA, Guatemala.

El Salvador

UNIVERSIDAD DE EL SALVADOR, San Salvador.

MUSEO NACIONAL "DAVID J. GUZMÁN," San Salvador.

DEPARTAMENTO TÉCNICO DE EXCAVACIONES ARQUEOLÓGICAS, San Salvador.

MUSEO DE TAZUMAL, Tazumal.

INSTITUTO INDIGENISTA NACIONAL DE EL SALVADOR, San Salvador.

Honduras

UNIVERSIDAD DE HONDURAS, Tegucigalpa, D.C.

MUSEO NACIONAL, Tegucigalpa.

MUSEO ARQUEOLÓGICO NACIONAL DE LAS RUINAS DE COPÁN, Department of Copán.

MUSEO DE COMAYAGUA, Comayagua.

MUSEO DEL DISTRITO, San Pedro Sula.

SOCIEDAD DE GEOGRAFÍA E HISTORIA DE HONDURAS, Tegucigalpa.

SOCIEDAD FOLKLÓRICA HONDUREÑA, Tegucigalpa.

SOCIEDAD DE ANTROPOLOGÍA Y ARQUEOLOGÍA DE HONDURAS, PROTECTORA DE LOS MONUMENTOS Y DOCUMENTOS NACIONALES, Tegucigalpa.

INSTITUTO NACIONAL DE ANTROPOLOGÍA E HISTORIA, Tegucigalpa.

Nicaragua

UNIVERSIDAD NACIONAL DE NICARAGUA, León.

MUSEO NACIONAL DE NICARAGUA, Managua.

MUSEO ARQUEOLÓGICO DEL INSTITUTO PEDAGÓGICO DE LOS HERMANOS CRISTIANOS, Managua.

MUSEO ARQUEOLÓGICO DEL COLEGIO CENTRO AMÉRICA, Granada.

MUSEO DE NINDIRI, Nindiri.

ACADEMIA DE GEOGRAFÍA E HISTORIA DE NICARAGUA, SOFONÍAS SALVATIERRA, Managua.

TALLER DE SAN LUCAS: COFRADÍA DE ESCRITORES Y ARTISTAS CATÓLICOS, Managua.

Costa Rica

UNIVERSIDAD DE COSTA RICA, San José.

MUSEO NACIONAL, San José.

MUSEO DEL COLEGIO SEMINARIO, San José.

JUNTA DE PROTECCIÓN DE LAS RAZAS ABÓRIGENES DE LA NACIÓN, San José.

INSTITUTO INTERAMERICANO DE CIENCIAS AGRÍCOLAS, Tuitialba.

ACADEMIA DE GEOGRAFÍA E HISTORIA DE COSTA RICA, San José.

Panamá

UNIVERSIDAD DE PANAMÁ, Panamá.

MUSEO NACIONAL DE PANAMÁ, Panamá.

ACADEMIA PANAMEÑA DE LA HISTORIA, Panamá.

CONSEJO INDIGENISTA NACIONAL DE PANAMÁ, Panamá.

COMISIÓN NACIONAL DE ARQUEOLOGÍA Y MONUMENTOS HISTÓRICOS, Panamá.

PANAMA SOCIETY FOR THE ADVANCEMENT OF ARCHAEOLOGICAL AND NATURAL SCIENCE, Balboa Heights, C.Z.

Cuba

UNIVERSIDAD DE LA HABANA, Havana.

UNIVERSIDAD DE ORIENTE DEPARTAMENTO DE EXTENSIONES Y RELACIONES CULTURALES, Santiago de Cuba.

MUSEO DE HISTORIA NATURAL DE LA ACADEMIA, Havana.

MUSEO ANTROPOLÓGICO MONTANÉ, Havana.

MUSEO DE ARQUEOLOGÍA E HISTORIA DE LA UNIVERSIDAD DEL ORIENTE, Santiago de Cuba.

MUSEO MUNICIPAL "EMILIO BACARDI MOREAU," Santiago de Cuba.

MUSEO "IGNACIO AGRAMONTE" Y BIBLIOTECA PÚBLICA "ISABEL ESPERANZA BETANCOURT," Camagüey.

MUSEO GARCÍA FERRI, Holguín.

MUSEO URBET, Manzanillo.

MUSEO "OSCAR M. DE ROJAS," Cárdenas.

MUSEO DE REMEDIOS "JOSÉ MARÍA ESPINOSA," Remedios.

ACADEMIA DE CIENCIAS MÉDICAS, FÍSICAS Y NATURALES DE LA HABANA, Havana.

SOCIEDAD DE HISTORIA NATURAL "FELIPE POEY," Havana.

SOCIEDAD DE ESTUDIOS HISTÓRICOS E INTERNACIONALES, Havana.

JUNTA NACIONAL DE ARQUEOLOGÍA Y ETNOLOGÍA, Havana.

GRUPO GUAMÁ, Havana.

ASOCIACIÓN DE ARQUEÓLOGOS DEL CARIBE, Havana.

GRUPO CAONABO, Motón.

SOCIEDAD ARQUEOLÓGICA DE BANES, Banes.

SOCIEDAD DE GEOGRAFÍA E HISTORIA DE ORIENTE "GRUPO HUMBOLDT," Santiago de Cuba.

SOCIEDAD DE ESTUDIOS AFRO-CUBANO, Havana.

British West Indies

UNIVERSITY COLLEGE OF THE WEST INDIES, Mona, St. Andrew, Jamaica.

INSTITUTE OF SOCIAL AND ECONOMIC RESEARCH, Mona, St. Andrew, Jamaica.

INSTITUTE OF JAMAICA, Kingston, Jamaica.

ROYAL VICTORIA INSTITUTE, Port-of-Spain, Trinidad.

THE HISTORICAL SOCIETY OF TRINIDAD AND TOBAGO, Port-of-Spain, Trinidad.

Haiti

INSTITUT D'ETHNOLOGIE D'HAÏTI, Port-au-Prince.

BUREAU D'ETHNOLOGIE DE LA RÉPUBLIQUE D'HAÏTI, Port-au-Prince.

LE MUSÉE DU PEUPLE HAÏTIEN, Port-au-Prince.

COLLECTION KURT FISHER, Port-au-Prince.

Dominican Republic

UNIVERSIDAD DE SANTO DOMINGO, Ciudad Universitaria, Ciudad Trujillo.

INSTITUTO DE INVESTIGACIONES ANTROPOLÓGICAS, Ciudad Trujillo.

MUSEO NACIONAL, Ciudad Trujillo.

Puerto Rico

UNIVERSIDAD DE PUERTO RICO, Río Piedras.

CENTRO DE INVESTIGACIONES SOCIALES, Río Piedras.

CENTRO DE INVESTIGACIONES ARQUEOLÓGICAS Y ETNOLÓGICAS, Río Piedras.

MUSEO DE ANTROPOLOGÍA, HISTORIA Y ARTE, Río Piedras.

Virgin Islands

ST. CROIX MUSEUM, Christiansted.

French West Indies

MUSÉE DE SEMINAIRE COLLÈGE, Fort de France.

Netherlands Antilles

HET CURAÇAOSCH MUSEUM, Willemstad.

British Guiana

BRITISH GUIANA MUSEUM, Georgetown.

Venezuela

UNIVERSIDAD CENTRAL DE VENEZUELA, Caracas.

MUSEO DE CIENCIAS NATURALES, Caracas.

SOCIEDAD DE CIENCIAS NATURALES "LA SALLE," Caracas.

SOCIEDAD DE CIENCIAS NATURALES "LA SALLE" DE VALENCIA, Valencia.

Colombia

PONTIFICIA UNIVERSIDAD CATÓLICA JAVERIANA, Bogotá.

INSTITUTO COLOMBIANO DE ANTROPOLOGÍA, Bogotá.

INSTITUTO ETNOLÓGICO NACIONAL DE BOGOTÁ, Bogotá.

INSTITUTO DE INVESTIGACIÓN ETNOLÓGICA, Barranquilla.

MUSEO ARQUEOLÓGICO DE LA UNIVERSIDAD DEL CAUCA, Popayán.

SERVICIO ETNOLÓGICO DE LA UNIVERSIDAD DE ANTIOQUIA, Medellín.

Ecuador

INSTITUTO ECUATORIANO DE ANTROPOLOGÍA Y GEOGRAFÍA, Quito.

MUSEO ARQUEOLÓGICO Y HISTÓRICO, Quito.

MUSEO ETNOGRÁFICO DE LA UNIVERSIDAD CENTRAL DEL ECUADOR, Quito.

MUSEO JACINTO JIJON Y CAMAÑO, Quito.

Peru

UNIVERSIDAD NACIONAL MAYOR DE SAN MARCOS, Lima.

Instituto de Etnología, Universidad de San Marcos.

ARCHIVO FOLKLORICO DE LA SECCIÓN DE FOLKLORE, BELLAS ARTES Y DESPACHO DE LA DIRECCIÓN DE EDUCACIÓN ARTÍSTICA Y EXTENSION CULTURAL DEL MINISTERIO DE EDUCACIÓN PÚBLICA, Lima.

INSTITUTO DE BIOLOGÍA ANDINA, Lima.

INSTITUTO DE ESTUDIOS ETNOLÓGICOS, Lima.

INSTITUTO INDIGENISTA PERUANO, Lima.

MUSEO ARQUEOLÓGICO DE ANCASH, Huarás, Ancash.

MUSEO HISTÓRICO REGIONAL DE AYACUCHO, Ayacucho.

MUSEO HISTÓRICO REGIONAL DE ICA, Ica.

MUSEO NACIONAL DE ANTROPOLOGÍA Y ARQUEOLOGÍA, Lima.

Bolivia

MUSEO NACIONAL TIAHUANACU, La Paz.

Chile

DEPARTMENT OF EXPERIMENTAL MEDICINE OF THE NATIONAL HEALTH SERVICE, Santiago de Chile.

INSTITUTO DE HISTOLOGÍA Y EMBRIOLOGÍA DE LA UNIVERSIDAD DE CONCEPCIÓN, Concepción.

INSTITUTO DE INVESTIGACIONES MUSICALES, Santiago de Chile.

MUSEO ARQUEOLÓGICO DE LA SERENA, La Serena.

MUSEO DE CONCEPCIÓN, Concepción.

MUSEO HISTÓRICO NACIONAL DE CHILE, Santiago de Chile.

MUSEO NACIONAL DE HISTORIA NATURAL, Santiago de Chile.

SOCIEDAD ARQUEOLÓGICA DE LA SERENA, La Serena.

SOCIEDAD CHILENA DE HISTORIA Y GEOGRAFÍA, Santiago de Chile.

Argentina

UNIVERSIDAD NACIONAL DE LA PLATA, La Plata.

Faculty of Natural Sciences and Museums of La Plata.

DEPARTAMENTO DE ESTUDIOS ETNOGRÁFICOS Y COLONIALES, Santa Fé.

INSTITUTO DE ANTROPOLOGÍA (MUSEO ETNOGRÁFICO), Buenos Aires.

INSTITUTO DE ANTROPOLOGÍA DE LA UNIVERSIDAD NACIONAL DE TUCUMÁN, Tucumán.

INSTITUTO DE ARQUEOLOGÍA, LINGÜÍSTICA Y FOLKLORE "DR. PABLO CABRERA," Córdoba.

MUSEO ARGENTINO DE CIENCIAS NATURALES "BERNARDINO RIVADAVIA," Buenos Aires.

MUSEO ARQUEOLÓGICO PROVINCIAL DE SANTIAGO DEL ESTERO, Santiago del Estero.

MUSEO DE HISTORIA NATURAL "JUAN CORNELIO MOYANO," Mendoza.

SOCIEDAD ARGENTINA DE AMERICANISTAS,
Buenos Aires.
SOCIEDAD ARGENTINA DE ANTROPOLOGÍA,
Buenos Aires.

Uruguay

MUSEO DE HISTORIA NATURAL DE MONTEVIDEO, Montevideo.

Brazil

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SÃO PAULO, São Paulo.
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pinas.
FACULDADE DE FILOSOFIA DE CURITIBA, Curí-
tiba.
FACULDADE DE FILOSOFIA E LETRAS DE JUIZ
DE FORA, Juiz de Fora.
FACULDADE DE FILOSOFIA DO RECIFE, Recife.
FACULDADE DE FILOSOFIA DE SÃO BENTO, São
Bento.
FACULDADE SALESIANA DE LORENA, Lorena.
UNIVERSIDADE DO PARANÁ, Curitiba.
UNIVERSIDADE DO RECIFE, Recife.
UNIVERSIDADE DO RIO GRANDE DO SUL, Porto
Alegre.
UNIVERSIDADE DE SÃO PAULO, São Paulo.
UNIVERSIDADE CATOLICA DE PERNAMBUCO,
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INSTITUTO DE PESQUISAS SOCIAIS "JOAQUIM
NABUCO," Recife.
MUSEU DO ESTADO DE PERNAMBUCO, Recife.
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Paulo.
CONCELHO NACIONAL DE PESQUISAS, Rio de
Janeiro.
FUNDAÇÃO PARA O DESENVOLVIMENTO DA
CIENCIA NA BAHIA, Bahia.

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BENNINGTON COLLEGE, Bennington, Ver-
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BLACK MOUNTAIN COLLEGE, Black Moun-
tain, N.C.
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BRYN MAWR COLLEGE, Bryn Mawr, Penna.
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COLGATE UNIVERSITY, Hamilton, N.Y.
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WILLIAM SMITH COLLEGES, Geneva, N.Y.
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London, Conn.
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COGNATE LEARNING, Philadelphia, Penna.
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Md.
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LAWRENCE COLLEGE, Appleton, Wis.
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Mass.
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OCCIDENTAL COLLEGE, Los Angeles, Calif.
OHIO STATE UNIVERSITY, Columbus, Ohio.
OKLAHOMA AGRICULTURAL AND MECHANICAL
COLLEGE, Stillwater, Okla.
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College, Penna.
PRINCETON UNIVERSITY, Princeton, N.J.
QUEENS COLLEGE, Flushing, N.Y.
REED COLLEGE, Portland, Ore.
ST. LAWRENCE UNIVERSITY, Canton, N.Y.
ST. MARTIN'S COLLEGE, Olympia, Wash.
SAN DIEGO STATE COLLEGE, San Diego, Calif.
SAN FRANCISCO STATE COLLEGE, San Fran-
cisco, Calif.
SKID LAWRENCE COLLEGE, Bronxville, N.Y.
SARAH MORE COLLEGE, Saratoga Springs, N.Y.
SOUTHERN ILLINOIS UNIVERSITY, Carbondale,
Ill.
STANFORD UNIVERSITY, Stanford, Calif.
THE STATE COLLEGE OF WASHINGTON, Pull-
man, Wash.
STATE UNIVERSITY OF IOWA, Iowa City, Iowa.
STETSON UNIVERSITY, Deland, Fla.
SYRACUSE UNIVERSITY, Syracuse, N.Y.
TULANE UNIVERSITY, New Orleans, La.
UNIVERSITY OF ALABAMA, University, Ala.
UNIVERSITY OF ALASKA, College, Alaska.
UNIVERSITY OF ARIZONA, Tucson, Ariz.
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Los Angeles, Calif.
UNIVERSITY OF CHICAGO, Chicago, Ill.
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Ohio.
UNIVERSITY OF COLORADO, Boulder, Colo.
UNIVERSITY OF CONNECTICUT, Storrs, Conn.
UNIVERSITY OF DELAWARE, Newark, Del.
UNIVERSITY OF DENVER, Denver, Colo.
UNIVERSITY OF FLORIDA, Gainesville, Fla.

UNIVERSITY OF GEORGIA, Athens, Ga.
UNIVERSITY OF HAWAII, Honolulu, Hawaii.
UNIVERSITY OF IDAHO, Moscow, Idaho.
UNIVERSITY OF ILLINOIS, Urbana, Ill.
UNIVERSITY OF KANSAS, Lawrence, Kan.
UNIVERSITY OF KENTUCKY, Lexington, Ky.
UNIVERSITY OF LOUISVILLE, Louisville, Ky.
UNIVERSITY OF MIAMI, Coral Gables, Fla.
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UNIVERSITY OF MINNESOTA, Minneapolis,
Minn.
UNIVERSITY OF MISSISSIPPI, University, Miss.
UNIVERSITY OF MISSOURI, Columbia, Mo.
UNIVERSITY OF NEBRASKA, Lincoln, Neb.
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Forks, N.D.
UNIVERSITY OF NOTRE DAME, Notre Dame,
Ind.
UNIVERSITY OF OKLAHOMA, Norman, Okla.
UNIVERSITY OF OREGON, Eugene, Ore.
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delphia, Penna.
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S.C.
UNIVERSITY OF SOUTHERN CALIFORNIA, Los
Angeles, Calif.
THE UNIVERSITY OF TENNESSEE, Knoxville,
Tenn.
UNIVERSITY OF TEXAS, Austin, Texas.
UNIVERSITY OF TULSA, Tulsa, Okla.
UNIVERSITY OF UTAH, Salt Lake City, Utah.
UNIVERSITY OF WASHINGTON, Seattle, Wash.
UNIVERSITY OF WISCONSIN, Madison, Wis.
VANDERBILT UNIVERSITY, Nashville, Tenn.
VASSAR COLLEGE, Poughkeepsie, N.Y.
WASHINGTON UNIVERSITY, St. Louis, Mo.
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WELLESLEY COLLEGE, Wellesley, Mass.
WESTERN RESERVE UNIVERSITY, Cleveland,
Ohio.
WESTERN WASHINGTON COLLEGE OF EDUCA-
TION, Bellingham, Wash.
WHEATON COLLEGE, Wheaton, Ill.
WILLAMETTE UNIVERSITY, Salem, Ore.
YALE UNIVERSITY, New Haven, Conn.

ALABAMA MUSEUM OF NATURAL HISTORY,
University, Ala.
ALASKA HISTORICAL LIBRARY AND MUSEUM,
Juneau, Alaska.
AMERICAN MUSEUM OF NATURAL HISTORY,
New York, N.Y.
AMERICAN SCHOOL OF PREHISTORIC RE-
SEARCH, Cambridge, Mass.
AMERICAN SCHOOLS OF ORIENTAL RESEARCH,
New Haven, Conn.
THE AMERIND FOUNDATION, INC., Dragoon,
Ariz.
ARCTIC, DESERT, TROPIC INFORMATION CEN-
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ARIZONA STATE MUSEUM, Tucson, Ariz.
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Texas.
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BUFFALO MUSEUM OF SCIENCE, Buffalo, N.Y.
BUREAU OF AMERICAN ETHNOLOGY, Wash-
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Washington, D.C.
CARNEGIE MUSEUM, Pittsburgh, Penna.
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cago, Ill.

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- CHINESE HISTORY PROJECT, Columbia University, New York, N.Y.
- THE CONSTITUTION LABORATORY, Columbia University, New York, N.Y.
- CRANBROOK INSTITUTE OF SCIENCE, Bloomfield Hills, Mich.
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- DAVENPORT PUBLIC MUSEUM, Davenport, Iowa.
- DENVER ART MUSEUM, Denver, Colo.
- DENVER MUSEUM OF NATURAL HISTORY, Denver, Colo.
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- FELS RESEARCH INSTITUTE FOR THE STUDY OF HUMAN DEVELOPMENT, Yellow Springs, Ohio.
- THE FLORIDA STATE MUSEUM, Gainesville, Fla.
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- FOLKLORE INSTITUTE OF AMERICA, Bloomington, Ind.
- GESSELL INSTITUTE OF CHILD DEVELOPMENT, New Haven, Conn.
- GRAND RAPIDS PUBLIC MUSEUM, Grand Rapids, Mich.
- THE HEARD MUSEUM, Phoenix, Ariz.
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- HUMAN RELATIONS AREA FILES, INC., New Haven, Conn.
- HUMAN RESOURCES RESEARCH INSTITUTE, Maxwell Air Force Base, Ala.
- ILLINOIS STATE MUSEUM, Springfield, Ill.
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- INSTITUTE FOR RESEARCH IN SOCIAL SCIENCE, Chapel Hill, N.C.
- INSTITUTE FOR SEX RESEARCH, INC., Bloomington, Ind.
- INSTITUTE FOR THE STUDY OF HUMAN VARIATION, New York, N.Y.
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- LABORATORY OF ANTHROPOLOGY AND ARCHAEOLOGY OF THE UNIVERSITY OF NORTH CAROLINA, Chapel Hill, N.C.
- LABORATORY OF TREE-RING RESEARCH, Tucson, Ariz.
- LIBRARY OF CONGRESS, Washington, D.C.
- LOGAN MUSEUM OF ANTHROPOLOGY, Beloit, Wis.
- LOS ANGELES COUNTY MUSEUM OF HISTORY, SCIENCE AND ART, Los Angeles, Calif.
- MENNINGER SCHOOL OF PSYCHIATRY, Topeka, Kan.
- MIDDLE AMERICAN RESEARCH INSTITUTE, New Orleans, La.
- MILWAUKEE PUBLIC MUSEUM, Milwaukee, Wis.
- MISSOURI RESOURCES MUSEUM, Jefferson City, Mo.
- MUSEUM OF THE AMERICAN INDIAN, New York, N.Y.
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- MUSEUM OF INTERNATIONAL FOLK ART, Santa Fe, N.M.
- MUSEUM OF MODERN ART, New York, N.Y.
- MUSEUM OF NATURAL HISTORY, Lawrence, Kan.
- MUSEUM OF NATURAL HISTORY, Eugene, Ore.
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- THE MUSEUM OF NEW MEXICO, Santa Fe, N.M.
- MUSEUM OF THE PLAINS INDIAN, Browning, Mont.
- MUSEUM OF SCIENCE, Boston, Mass.
- NATIONAL ARCHIVES AND RECORDS SERVICE, Washington, D.C.
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- NEVADA STATE MUSEUM, Carson City, Nev.
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- NEW JERSEY STATE MUSEUM, Trenton, N.J.
- NEW YORK STATE MUSEUM AND STATE SCIENCE SERVICE, Albany, N.Y.
- THE NEWARK MUSEUM, Newark, N.J.
- THE NEWBERRY LIBRARY, Chicago, Ill.
- NORTHERN ARIZONA SOCIETY OF SCIENCE AND ART, INC., MUSEUM OF NORTHERN ARIZONA, Flagstaff, Ariz.
- OAKLAND PUBLIC MUSEUM, Oakland, Calif.
- OHIO STATE ARCHAEOLOGICAL AND HISTORICAL SOCIETY, MUSEUM AND LIBRARY, Columbus, Ohio.
- OLD COURT HOUSE STATE MONUMENT, Lincoln, N.M.
- THE ORIENTAL INSTITUTE OF THE UNIVERSITY OF CHICAGO, Chicago, Ill.
- PEABODY MUSEUM OF ARCHAEOLOGY AND ETHNOLOGY, HARVARD UNIVERSITY, Cambridge, Mass.
- PEABODY MUSEUM OF NATURAL HISTORY, YALE UNIVERSITY, New Haven, Conn.
- PEABODY MUSEUM OF SALEM, Salem, Mass.
- PHILADELPHIA CENTER FOR RESEARCH IN CHILD GROWTH, Philadelphia, Penna.
- PORTLAND ART MUSEUM, Portland, Ore.
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Our Readers Write



Anyone in the United States who has taught students of anthropology or has been a graduate student (especially since World War II) knows that our discipline is peculiarly incestuous. Unless the male graduate student is blessed with a completely understanding wife who shares his interests and is willing to forego plumbing—or else will play the patient Griselda for long periods of time—he may regard bachelorhood as preferable to wedded misery. The female graduate student who wants both marriage and field research can seldom obtain these goals without marrying another anthropologist; and many promising women have had to abandon anthropology because they practiced exogamy.

At the same time, field work is frequently best carried out by teams representing both sexes, since two sexes will usually be found among the group studied. In some cases, it is actually impossible to obtain firsthand information from persons of the opposite sex. Even where such cultural barriers are absent or minimal, the data may still be skewed. For example, anthropological literature includes many studies of marriage systems where Ego marries his Mother's Brother's Daughter. How many studies recognize female Ego, who in the same situation is marrying into her Father's Sister's family? And is this patrilineal or matrilineal cross-cousin marriage? For these and other reasons, anthropologists encourage male–female field research.

Yet our own cultural patterns, and sometimes those encountered in the field as well, encourage distrust and suspicion of male–female teams absent from home for long periods, unless their association has the sanction of marriage. Consequently, graduate students and teachers consider an endogamous mating pattern well-suited to the profession of anthropology, and such unions are frequently encouraged.

How often, however, do we also take into account what happens between field trips? At most U.S. universities, only one anthropologist at a time can be hired as a member of the faculty.

Furthermore, university regulations usually prohibit a department's hiring two members of the same family. One member of this highly-endorsed team is thus automatically barred from pursuing aspects of anthropology other than field work, unless the team happens to live where there are several universities. He or she cannot go back into the field, or be affiliated with a teaching staff; and lacking such affiliation, is unlikely to receive outside support for research. While one member teaches, the other is generally forced to remain unemployed professionally.

Should we issue temporary marriage licenses, good for the duration of field research and automatically annulled upon return? Should we encourage graduate students to take vows of celibacy, since out-group marriage presents too many difficulties while in-group marriage results in the waste of half the team? Or should anthropologists, as members of a profession peculiarly affected by this problem, seek channels for utilizing the energies and information of the dormant partners?

BEATRICE MILLER
Madison, Wisconsin, U.S.A.

The position of the young English anthropologist who thinks the money used for CA could be better spent for other purposes seems unjustified; from the viewpoint of anthropologists in the smaller nations it could even be called egotistical. To an anthropologist in one of the larger countries, who has many journals at his disposal, is able to travel, and can easily correspond with institutions in other countries, CA might be of little value. But anthropologists in small nations lack these advantages; and the appearance of an international journal of anthropology such as CA is an important event for us. Such a journal offers us the opportunity to maintain contact with all our colleagues throughout the world, and to obtain current information about the progress of the anthropological and ethnological sciences.

For this reason, and in accordance with an exchange of opinions with H.

Vallois, I am convinced that CA should—officially or unofficially—assume the role of an organ of the International Union of Anthropological and Ethnological Societies, which at present lacks such an organ. Naturally, CA cannot accept all suggestions for its further development (as in the tale about the man, his son, and the donkey); but I feel it my duty to present the view of an anthropologist from a small nation where conditions of work are different.

To further the development of CA as an international journal that would report developments in anthropology throughout the world, I think that real progress could be achieved by appointing an editor for each country. Such an editor would send CA current news about anthropological research in his country, contact subscribers, and assign specialists the preparation of articles for CA after co-ordinating plans with the Editor of CA. Such contributing editors would not be paid, but might receive a *gratis* subscription to CA.

The department "Our Readers Write," I think, has carried too many opinions about whether CA should exist or not. By now it is obvious that CA is very useful and that it should exist. In the future, discussion of this issue should be confined within the editorial board of CA, and not published. "Our Readers Write" should be limited to proposals for writing articles, so that readers would be informed about problems to be discussed in future numbers of the journal.

With time, I imagine CA would acquire the character of a society whose members would feel the moral obligation to answer when being consulted, and to send reprints and photocopies to their colleagues when asked.

PETER BOEV
Sofia, Bulgaria

I suggest that "Publications Received" be a regular feature of CA and that, to extend the service of the *Fellow Newsletter* of the American Anthropological Association, there also be regular provision for requesting field work, research opportunities, and employment in other institutions, on an international basis.

ALLEN SPITZER
St. Louis, Mo., U.S.A.

The suggestions and comments on abstracts in CA (1: 346; 2: 26) deserve consideration, since abstracts in several widely-used languages would do a great deal to offset the disadvantages of publication in English alone.

I strongly disagree with the statement that abstracts have little value for conveying knowledge or ideas; when carefully written, they convey the essential

points made by an author—and I think we can assume that all CA articles will have points worth summarizing. Abstracts printed in CA would also be available for reprinting or for filing in specialized indexes (as two readers suggested). With the volume of anthropological publications increasing so greatly, there must eventually (and the sooner the better) be a thorough coverage in annual volumes of abstracts, such as most of the physical sciences have long had. The inclusion of abstracts in CA would help demonstrate their value.

At the least, I would urge brief abstracts in English, French, German, and Spanish. The 14 major articles in Volume I would have needed no more than 14 pages for these, adding about 3% to the length of the volume.

RICHARD B. WOODBURY
Tucson, Ariz., U.S.A.

I was much interested in A. Sharma's letter (February 1961, p. 1) about the

exchange of museum materials. It seems to me that CA could act as a clearinghouse for preliminary information (statements about the articles wanted, or those available for exchange), and that the details of the transaction could then be settled by personal letters between the interested parties. Colgate University has recently acquired a large collection of Iroquois archaeological material, mostly dating from the 16th and 17th centuries A.D. When the collection has been catalogued, a number of reconstructed pottery vessels, bone and stone artifacts, and burial furniture from the European contact period should be available for exchange. In return, we should like to acquire ethnographic specimens from Asia, Africa, or Oceania. We use our collections mainly for classroom demonstration, and for comparative purposes in exhibits, so we are not too much concerned about the exact provenience or complete representation of types. Later this year I

shall be able to send photographs and descriptions of our exchange material to anyone interested.

JOHN M. LONGYEAR III
Hamilton, New York, U.S.A.

Conklin's paper on shifting cultivation (CA February 1961) reminds me of the techniques used in the old Scottish farming method of infield-outfield cultivation, with transhumance to the upland grazings for the beasts. Both the outfield and the grazing were frequently cleared by burning. Dr. Estyn Evans in Belfast might be consulted, for there is a good bridge here between the Atlantic-zone peasant agriculture and the tropical practices. Field studies of Scottish practices have been made by the Institute of Scottish Studies at the University of Edinburgh, as well as by Queen's University, Belfast.

JOHN MOGEY
Nashville, Tenn., U.S.A.

Calendar

1961

- July 3-9. 1st Caribbean Conference on Pre-Columbian Archaeology. Fort-de-France, Martinique. Write: Historical Society of Martinique, Fort-de-France.
- Aug. 21-Sept. 6. 10th Pacific Science Congress. Honolulu, Hawaii, U.S.A. Write: H. J. Coolidge, Bishop Museum, Honolulu 17.
- Aug. 28-Sept. 3. Intn'l Folk Music Council. 14th Annual. Quebec, Canada. Write: Miss Maud Karpeles, 35 Princess Court, Queensway, London W2, England.
- Sept. 6. 2nd Conference of the Intn'l Committee for Standardization in Human Biology. Rome, Italy. Write: G. A. Heuse, Intn'l Inst. of Human Biology, 27 rue du Fg-St-Jacques, Paris 14, France.
- Sept. 6-11. 8th Congress, Intn'l Musicological Society, New York, U.S.A. Write: Ernst Mohr, Case Postale 154, Basle 1, Switzerland.
- Sept. 7-12. 2nd Intn'l Conference of Human Genetics. Rome, Italy. Write: Istituto G. Mendel, 5, Piazza Galeno, Rome.
- Sept. 28-Oct. 1. 5th Conference of Czechoslovak Anthropologists. Milukov near Brno, Czechoslovakia. Write: Milan Dokládál, Anthropological Society, Komenského nám. 2, Brno.

teria que abarca, siempre que se observen razonables normas selectivas. Las ilustraciones son convenientes.

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- Nov. 16-19. American Anthropological Association. 60th Annual. Philadelphia, Pa., U.S.A. Write: John Landgraf, New York University, 413 Vanderbilt Hall, New York 3, N.Y., U.S.A.
- Dec. 28-30. American Association for the Advancement of Science. Section H (Anthropology) annual meeting. Denver, Colo., U.S.A. Program Chairman, D. M. Pendergast, Dept. Anthropology, Univ. of Utah, Salt Lake City, Utah, U.S.A.

1962

- Aug. 29-Sept. 3. 6th Intn'l Congress of Prehistoric and Proto-historic Sciences. Rome, Italy. Write: L. Cardinali, % Università degli Studi, Rome.
- Sept. 2-8. 5th World Congress of Sociology. Washington, U.S.A. Write: Intn'l Sociological Assn., Skepper House, 13 Endsleigh St., London W.C. 1, England.
- September. 9th Intn'l Congress of Linguists. Cambridge, Mass., U.S.A. Write: Miss Chr. Mohrmann, 40 Sint Annastraat, Nijmegen, Netherlands.
- Nov. 15-17. American Anthropological Association. 61st Annual. Chicago, Ill., U.S.A.

las bibliografías impresas, repertorio de repertorios de materiales de investigación, directorios de especialistas e instituciones científicas en campos importantes, listas de tesis doctorales, investigaciones en curso, así como toda clase de información útil. A ello se añadirán peticiones de información necesaria para proseguir trabajos concretos y sugerencias y discusión de investigaciones a emprender.

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